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MONISM AND DUALISM:

NEW LAMPS FOR OLD

THE PRESIDENTIAL ADDRESS AT THE ONE HUNDRED AND TWENTY-FIRST ANNUAL MEETING OF THE ROYAL MEDICO-PSYCHOLOGICAL ASSOCIATION HELD AT QUEEN'S UNIVERSITY, BELFAST, ON 5 JULY, 1961

By

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THE Presidency of this Association carries with it an opportunity for service commensurate with the challenge of the time, and I am grateful that you should think me worthy of this high honour and fit for such responsibility at a crucial phase in the development of psychiatry. Events will show whether you have been wise in your choice, but however that may be you can be sure of my utmost effort on behalf of the Association.

One of the first duties of a newly-installed President is perhaps his greatest ordeal—the Presidential Address, with its problems of choice of topic, selection of a suitable title and presentation of subject matter. This is no problem to some, but I share the perplexity of a number of my predecessors.

When Dr. Michael J. Nolan was President on the only other occasion on which the Annual Meeting has been held in Northern Ireland, he commented on the difficulty of finding a subject which is both novel and of common interest.

My subject has not been of general interest to psychiatrists, although by the same token it is novel, since dualism-monism, if touched on at all, is rarely discussed in psychiatric text books or literature—even in psychosomatic contributions; an exception to this is the Presidential Address of Dr. Stanley Cobb to the American Psychosomatic Society in 1956 (1). In my view it is a pity that psychiatrists have not given more attention to this matter since, apart from the intrinsic importance of the central issue of mind-brain relationships, it has considerable influence on current thought and outlook.

Neurologists do not share our diffidence in discussing mind-brain relationship and it is more probably fortuitous that the subject was first chosen in the 13th of the series of Hughlings Jackson Lectures (2), rather than that its previous avoidance had been, as Sir Francis Walshe suggests, due to discretion on the part of his predecessors.

Neurologists join physicists, philosophers and physiologists in what might be described as applied metaphysics to the extent that a recent writer in the *Lancet* (3) coined the term "neurosophy" for a cult of the last two or three decades.

I may be showing less than average discretion in setting out to have a look at the sense and sequence of the erudite, at the historical setting of the controversy and its effect on contemporary thought. There is ample scope for other psychiatrists to do better. My survey must perforce be of the most sketchy nature because the literature is vast.

The metaphysical approach is well over 2,000 years old, the scientific about a hundred, or three hundred at the most if one includes René Descartes' speculative science.

Metaphysical concepts are as diverse as they are personal, controversial and inconclusive, and the depth of much of the literature is a tax on the understanding of the reader. The philosopher is, as Sir Russell Brain (4) has pointed out, unfamiliar with problems which daily confront neurologists and psychiatrists. His ideas of mind-brain relationship can therefore have only limited direct application to these specialties. Occasionally however the philosopher will show the way, as when Herbert Spencer inspired Hughlings Jackson's work. Even when some writing is so abstruse as to be barely intelligible it may indicate new avenues of thought, as when Immanuel Kant showed that time and space are not inherent in things although applicable to our experience of them; an idea which is highly relevant to any attempted physical concept of mind.

Whatever contribution metaphysics has made, much of it is naïve in the light of modern medical and scientific knowledge, but in turning away from metaphysics much modern thought has turned its back on philosophy as a whole and has thereby lost a guide and control in speculation.

I feel that the 20th century errs more in this respect than the 19th, and do not agree with Sir Francis Walshe (2) when he talks of "... the inevitable fruit of the divorce of natural science from metaphysics to have achieved which was the empty triumph of the 19th century".

It seems to me that leaders of thought in the 19th century as represented by Thomas Huxley, John Tyndall, Henry Maudsley, Hughlings Jackson and Francis Galton were able to combine a broad scientific outlook with philosophic insight and profundity of thought with lucidity of expression, whereas increasing specialization of the 20th century is, to some extent at any rate, offsetting scientific advance by narrowing perspective.

Broadly speaking, dualism postulates the co-existence of body and mind, monism their continuity and identity. In Descartes' dualism the divide was absolute and unbridgeable but it is probably true to say that dualist thought now conceives contact and concomitance between, but not continuity of, mind and body; to the monist, however, mind seems to be merely a complicated integration of known patterns in the central nervous system.

This is an over-simplification. In metaphysics there are two dualisms of mind and matter, heterogeneous and hypothetical. According to the former both mind and matter are each absolute; the latter holds that, although mind cannot bridge the chasm to know matter, it is compelled by its own laws of cause and effect to postulate matter as the origin if not the cause of its sensations (5). Hypothetical dualism is perhaps the one most favoured today. According to it consciousness is of two kinds, sensation and reason, the latter entirely separate from all the data of experience.

I should interpose here that no comment of mine this afternoon should be

taken as having any theological implication even when, in discussing dualism, I refer to Descartes' identification of soul with mind and his location of soul in the pineal body; or when I mention the paradox of what appears to me to be the liberalization of religious thought in the last 80 years—a liberalization which strengthens, in contrast with a tendency to dogma and intolerance in certain scientific circles which must impair objectivity.

Monism is much younger as a philosophy than dualism (6), having been introduced by Wolff in Germany about the beginning of the 18th century as the opposite of dualism. It was felt that reality is just one substance and that everything is merely part of that ultimate substance. The monism which identifies ultimate reality with matter or physical energy is materialistic monism; that which identifies ultimate reality with mind or spirit is psychical monism. Many other varieties appear to me to be of purely metaphysical interest. Monism in metaphysics moves in circles of meaning, some parts being barely distinguishable from dualism, e.g., Spinoza's logical monism which combines more than one irreducible minimum. There is even a dualistic monism which is neither material nor physical but expresses itself in these parallel ways.

Dualism should be considered against what Thomas Huxley (7) described as "the parallax of time" which as he says "helps us to the true position of a conception as the parallax of space helps us to that of a star".

Dualism is associated more with the name of René Descartes than anyone else. Apart from his religious upbringing Descartes' concept of soul and body was inherited through the Scholastics and medieval Churchmen from Greek thought, and this in turn derived from the ancient Orphic cult and from the effect on Greek thought of the Zoroasterian mystic dualism of spirit and matter with which Alexander's soldiers came into contact in the Persian campaigns. Dualism has influenced human outlook since—it marks thought at every stage of recorded history.

Runes quotes Whitehead as saying that Descartes' dualism "poisoned all subsequent philosophy" and Bronowski and Mazlish (8) say that it is ironical that the man who introduced the idea that the structure of the earth is mathematical and logical should have introduced into modern philosophy "the dualism of mind and body which has plagued thought ever since". However Sir Geoffrey Jefferson (9) found Descartes' views very apposite to the present day, which according to him has become "more Cartesian than it realizes". The truth inherent in both these apparently conflicting statements lies in the fact that Descartes stood between classical and modern thought in that his concept of soul and body approximated to that of Plato and Aristotle, whereas his idea of the universality of mathematics and his concept of the analogy of mechanical activity with bodily function anticipated cybernetics.

There is nothing unusual in the paradox of Descartes' idealistic dualism with mechanistic outlook. For example, Bertrand Russell (10) describes Pythagoras as a combination of Einstein and Mrs. Eddy, and we find anomalies of thought nearer our own time. Thomas Huxley was agnostic but rejected materialism, Pavlov was militantly materialistic but is said to have resented being described as a mechanist (11). There is therefore no need to invoke the derogatory to explain Descartes as Pavlov (12) has done when he attributes Descartes' alleged inconsistency to an expediency in avoiding offence to the Church.

One might have expected more uniformity of outlook and conformity of opinion as science advanced its frontiers and consolidated its gains. If anything, the reverse has been the case and controversy has heightened. At the same time

theological attitude to scientific discovery is altering sensibly, has become more liberal and is no longer on the defensive. The paradoxical situation is, therefore, that whilst religion now seems to accept scientific discovery with equanimity and even welcome, science in some quarters in relation to mind-brain relations is becoming dogmatic and resentful of other views. Thomas Huxley (7) said nearly a century ago in this University: "The growth of science has not introduced new difficulties of principle into theological problems, it has merely given visible body, as it were, to those already existing."

According to A. N. Whitehead (13) religion and science have always been in a state of continual development and "there has always been conflict between religion and science". I agree with the former but as to the latter, in the main, it seems to be past tense as far as theologians are concerned.

Religion was still suffering from the shock of the publication of Charles Darwin's book *The Origin of Species* in 1859 when Wilberforce, Bishop of Oxford, attacked Huxley in 1860 with an insulting gibe about his ancestry, but 14 years later when Tyndall delivered his Presidential Address to the British Association here in Belfast (1874), although it was attacked in some quarters, condemnation was probably already less unanimous, as shown by the fact that one religious paper went so far as to describe it as "patting religion on the back".

Despite the relevance to current thought of views such as those of Kant and Bergson on space and time it is perhaps true to say that on the whole science began to replace metaphysics with Herbert Spencer's concepts on which so much of Hughlings Jackson's work on the evolution and dissolution of the central nervous system was based. The change seems to coincide more or less with the appearance of modern neurology, which has moved the divide between body and mind from between body and mental function as a whole—or soul, as Descartes would have said—to that between the physical basis of perceptual function, emotion and memory on the one hand, and on the other hand the realm of conceptual thought including aesthetics and ethics.

But the divide remains; it has only shifted, and in this sphere, when he speaks beyond clinical experience, the psychiatrist or neurologist does so with no greater authority than the philosopher. Lord Samuel (14) says: "The meeting place between mind and matter in our own experience is not where we had supposed it to be; it is not at the boundary between body and not body, but is internal. This, however, does not solve the problem, it merely shifts it."

Thomas Huxley is, I suppose, usually taken to epitomize an aggressive scientific outlook and to have held materialist views, which in fact he and for that matter Tyndall and Hughlings Jackson all repudiated. Huxley got his reputation from his reaction to attacks launched in the name of the Church, as when he reproved Wilberforce or talked (7) of "being deafened by the tattoo of 'the drum ecclesiastic' ". But he was attacking individuals and the pretensions of individuals and not the Church as such nor the sincere belief of others. He disputed the idea (7) that ecclesiastical eminence conferred authority to sit in judgment on scientific matters, but this was a general principle against any eminence which presumed beyond its experience. He did not deprecate the views of others, however robustly he may have disagreed. Thomas Huxley and John Tyndall seem to have what Bertrand Russell (15) had in mind when he said that it is one of the rarest gifts to be able to hold a view with conviction and detachment at the same time. Huxley somewhere refers to science as the Cinderella and theology and philosophy as the Ugly Sisters and he related consciousness with molecular change (7), but four years earlier he had told the

Cambridge Young Men's Christian Society in a lecture on Descartes' Discourse on Method that "when materialists stray beyond the borders of their path and begin to talk about there being nothing else in the Universe but Matter and Force and Necessary Laws . . . I decline to follow them" (7).

His biographer, William Irvine (16), seems to me to misinterpret Huxley when he says "he threw away the mind in order to preserve the brain in all the sharp precise integrity which his faith and his appetite for clearness demanded".

Huxley's contemporary and friend, Tyndall, showed that he believed in psycho-physical parallelism when he said (17) in his Presidential Address to the British Association here in 1874, "We can trace the development of the central nervous system and correlate with it the parallel phenomena of sensation and thought. We can see with undoubting certainty that they go hand in hand. But we try to soar into a vacuum the moment we try to comprehend the connection between them." This is very like Sir Russell Brain's description of mind as "the great amphibian . . . for though it is linked through the brain to the world of matter it moves in its own sphere as though it could soar above the physical" (18).

I should interpose here that Sir Charles Sherrington (14) said of Tyndall: "The oracular Professor Tyndall, presiding over the British Association in Belfast, told his audience that as the bile is a secretion of the liver, so the mind is a secretion of the brain." Such naïve hypothesis is quite out of keeping with Tyndall's breadth of perspective. I can find no reference whatever in his Address to such a view—to what Maudsley had described (19) seven years earlier as "the crude proposition of Cabanis that the brain secretes thought as the liver secretes bile" which according to Maudsley had "been a subject of much ridicule to those who have not received it with outcries of disapprobation and disgust".

Hughlings Jackson (20) was likewise dualistic in outlook and quotes Herbert Spencer with approval as insisting on the absolute distinction between states of consciousness and nervous states. Jackson's views of brain-mind relationship was one of contact without continuity, "a parallelism between a certain physical evolution and the correlative psychical evolution". In laying down an absolute distinction between states of consciousness and nervous states Jackson warned against "a materialism . . . as to mind which is not materialist at all". At the same time he recognized that, whether high or low level, nervous tissue is basically identical and "it would be marvellous if at a certain level whether you call it evolution or not these (nervous centres) were changed into centres of a different kind of constitution".

Whereas Tyndall and Hughlings Jackson believed in a parallelism which is dualistic, Huxley and Henry Maudsley were epiphenomenalists (21), i.e., mind was regarded as something incidental to the organism in which it occurs—a materialistic monism.

I have considered Tyndall, Huxley and Jackson at some length as it seems to me that the bifurcation of materialistic and idealistic thought inherent in Descartes' thinking was little less marked in some biological thought in the 19th century.

As I have already mentioned, the word dualism appears in few psychiatric textbooks but Jelliffe and White (22) in the 5th edition of their textbook *Diseases of the Nervous System* describe dualism as the quite arbitrary separation of the individual into two distinct and mutually exclusive parts—body and mind.

This is another oversimplification and is misleading since, in dualism,

however separate body and mind may be, they are *not* mutually exclusive. Indeed, although dualism and monism are opposites, their one point in common is a holistic view of the individual and his setting. Erwin Schrodinger, Professor of Physics in Vienna (23), refers to "this one thing, mind and world". Although to me Schrödinger's outlook appears to be dualistic, this quotation seems to come very close to that of Spinoza's "logical monism" (10) which conceives the world as "a single substance, none of whose parts are logically capable of existing alone".

Another cause of misunderstanding in this controversy may be the identification of the problem of dualism-monism with vitalism-mechanism, on which the discussion is whether living and inanimate are continuous. Sluckin (24) points out that these problems are logically separate. One is not necessarily the counterpart of the other. The assumption that they are identical may perhaps be part of the apparent paradox of Descartes' dualistic outlook—a belief in soul and animal automatism.

Sir Charles Sherrington (25) led 20th century dualistic thought. He described mind and brain as "disparate; not mutually convertible; untranslatable the one into the other". He also gave a new slant to the holistic philosophy of Jan Smuts (26), which regards mind as a whole which is greater than the sum of its parts. Sherrington's holism (25) regards the mind as "other than" instead of "greater than" the sum of its parts.

Before he gave the Gifford Lectures in 1937—published in 1938 as "Man On His Nature"—Sherrington had already been the subject of a vituperative attack by Pavlov because of dualistic views in *The Brain and its Mechanism*. A stenographic record (12) of one of Pavlov's Wednesday seminars quotes him as saying, "Gentlemen, can any of you, who have read Sherrington's booklet, say anything in defence of the author? I believe this is not a matter of some kind of misunderstanding, thoughtlessness or misjudgment. I simply suppose that he is ill, although he is only 70 years old, and that there are distinct signs of old age, of senility."

This is a measure of the intolerance of which a great mind is capable. Pavlov's question was, of course, purely rhetorical—no one at the Conference did, or was expected to, say anything in defence of Sherrington.

Coming nearer home we find Professor Gilbert Ryle announcing in the opening pages of his book *The Concept of Mind* (27) his intention of speaking often of what he describes as the Descartes' myth with "deliberate abusiveness as the 'dogma of the Ghost in the Machine' ", and saying, "I hope to prove that it is entirely false and false not in detail but in principle. It is not merely an assemblage of particular mistakes. It is one big mistake and a mistake of a special kind. It is, namely, a category mistake." In another contribution (14) he concludes that "'Mind' and 'matter' are echoes from the hustings of philosophy and prejudice the solution of all problems posed in terms of them."

The attitudes of Pavlov and Ryle are surely the opposite of conviction held with detachment which is so important in scientific and philosophic outlook.

Sir Francis Walshe (2) refers to "the chill physico-mathematical concept of the human mind" as "a muddy vesture of decay" and Lord Samuel (14) warns against addiction to what he describes as "the philosophic craving for unity".

Descartes' views on body-mind relationship were combined with what is to us a mediaeval physiology of hollow nerves, movement of animal spirits, the concentration of blood vessels around the imagined seat of the soul, the con-

version of the most active and quickest particles of blood into animal spirits. He is described as an unwearied dissector but his dualism was not influenced by this or by what was already known anatomically and physiologically. For example, his outlook was untouched by Vesalius' work on cerebral anatomy 100 years before or by Harvey's discovery of the circulation of blood which took place during his own lifetime.

It may be that the issues of dualism monism were more clear-cut against the sharp dualism of Descartes or in the parallelistic outlook of Tyndall and Hughlings Jackson than they are today against the background of modern neuro-anatomy, neuro-physiology, histochemistry, biophysics, current cybernetic analogy of mind and machine and Pavlov's study of higher nervous activity. Concepts such as reverberating circuits as a possible physical explanation of memory and the application of the electronic principle of negative feedback to the central control of goal-seeking activity, an understanding of the physiology of emotion and increasing knowledge of the physical basis of perception are tending to shift the divide between body and mind and leaving it less clearly defined.

To Cobb (28) "it can be maintained philosophically that an understanding of the physiology of perception is the basis of psychology and hence of psychiatry". He regards mind as an abstraction denoting the active integration of billions of nerve cells and hundreds of nuclei in the living brain, "it is merely so complex it is difficult to comprehend" which is consistent with his view that "the ultimate goal of science is a universal monism" (1).

The reverberating circuit theory is incidentally not a 20th century notion. Huxley (7) anticipated it in his Address to the British Association here 87 years ago when he said: "Hartley supposes that the vibrations excited by a sensory or other impression do not die but are represented by smaller vibrations", and added "Haller has substantially the same idea but contents himself with the general terms 'mutationes' to express the cerebral change which is the cause of a state of consciousness. These 'mutationes' persist for a long time after the cause which gives rise to them has ceased to operate . . .".

It is difficult to keep up with Ritchie Russell when in relation to the frontal lobes he says (29), "Here perhaps we find the physiology of the psychoanalyst's super ego, a vast array of power in certain cell layers of the cortex built up by the physiological elaboration of the hypothalamic home of primitive instincts."

Russell thus finds a place in his mechanism for psychotherapeutic concepts in contrast with the attitude of Warren McCullough who according to Sluckin (24) describes "what are known as the analytic or psychodynamic theories in medical psychology as so much nonsense". McCullough (24) apparently believes that "the functional psychoses are due to improper voltages at nerve cells which function like local-battery-operated electro-magnetic relays; neuroses occur when some of the negative feed-back loops become positive and regenerative". One wonders what essential difference is there between such reasoning on mind-brain relations and the often derided conclusions of Descartes on soul and body 300 years ago?

Attempts to formulate "mind" within the compass of physical concepts are unrealistic. Erwin Schrödinger (23) says, "This one thing—mind and world—may well be capable of other forms of appearance that we cannot grasp and do not imply notions of space and time." As already mentioned, Kant held that time and space are "not inherent in the objects of our knowledge but are something we put into them by our methods of observation" (30) and Ryle

points out that any fear of what he describes as the "Bogey of Mechanism" is baseless since "not all questions are physical questions" (27).

The views of Sir Russell Brain on perception—the importance of an understanding of which Cobb has emphasized in this context—have been dealt with in many publications such as *Mind, Perception and Science* (4), *The Nature of Experience* (31) and *Perception and Imperception* (32). This latter was the Thirtieth Maudsley Lecture and in it he remarked that it is an error in semantics if, either on the monist or parallelist basis, one believes that physical laws regulating the nervous system approximate to the mind.

The rarity of an ability to hold views with conviction and detachment and the frequent lack of attention to the history of thought are reasons why discussion often turns to controversy leading to anathemas.

Another difficulty is that soul and mind are frequently used ambiguously—synonymously or differentially without indication. The *Encyclopaedia Britannica* (14th edition) does not help, in that soul is not even mentioned in its Index. The three souls of the Ancients—the vegetative, the sensitive and the reasoning—persist in the structure of our language and soul may be taken to refer to either the immaterial part of man, the disembodied spirit, the moral or emotional part of man or the intellectual part of man. Descartes used it in the sense of the latter two in his physiological writing, though its other significance was always in his thought. The *Epigram* by Francis Quarles is a classic example of the identification of soul with mental function:

"My soul, sit thou a patient looker on;
Judge not the play before the play is done.
Her plot hath many changes; every day
Speakes a new scene; the last Act crowns the play."

Further confusion arises from the fact that mind is talked and thought of as a substantive—as a thing—when in fact, as Asher (33) has pointed out, it is merely a convenient term to embrace thinking, perceiving, memorizing, consciousness, ethics, aesthetics and similar attributes of living. Practical evidence of this confusion is seen in medico-legal relations in that lawyers seem to find it difficult to arrive at a common understanding of mind with doctors who are inclined to think of it more dynamically.

In *The Concept of Mind* Professor Ryle (27) comments on the variety of meaning attached to the words "conscious" and "consciousness", e.g., discovered, embarrassment, heeding, sentience and the Freudian idea of the unconscious. There is little doubt that many difficulties and misunderstandings in this matter and in psychiatry in general would be lessened if more attention were devoted to that branch of philology which is concerned with meanings.

Eliot Slater (14) says, "The ambiguities and contradictions which are involved when we make use of such words as 'consciousness', 'mind', 'free will' which now seem so insuperable, may yield to quiet investigation, or may be shown to be but verbal and the result of our asking ourselves the wrong sort of question. What we already know does however suggest that the relationship between body and mind is so intimate that they are best regarded as one."

Thought is objective in proportion to the degree that allowance is made to offset the influence of conditioned thinking and in this respect one notes the statement of Koshtoyants (12) in the introduction to Pavlov's *Selected Works* that Pavlov was "brought up on the tradition of Russian materialist philosophy and the spirit of militant irreconcilability to idealism". This to me seems to

be a declaration of a conditioned and dedicated search for uniformity. There is a determinism implicit in such outlook in which individuality is entirely the outcome of environmental factors and inborn attributes; in which outlook and philosophy—if it can be so called—is merely “an abstraction from an autobiography”.

A major difficulty in the consideration of mind-brain relationships is that, as Whitehead (13) says, “We cannot think in terms of an indefinite multiplicity of detail.” Tyndall gave an example of this when he pointed out (17) that even Aristotle and Goethe, who had an almost superhuman power of amassing and systematizing facts, could not see the force of mechanical reasoning. It is probable that for the same reason any comprehensive approach by an individual to psychiatry, such as the Meyerian, cannot be other than vague.

The metaphysics of dualism, monism, epiphenomenalism, vitalism, mechanism, have, by and large, been outmoded by a philosophy which is holistic whether it be an atheistic rejection of “the ghost in the machine”, an agnostic parallelism or, in the words of Tyndall (17), “a religious vitalization of the latest and deepest scientific truth”.

Increase in knowledge of the structure and mechanism of the central nervous system since Descartes' day has not brought any greater basic understanding of the relationship of mind and brain. We may, as Grey Walter (34) says, be “in a pre-Newtonian age, as it were, in which one can still argue amiably about Cartesian vortices though we may feel that any moment a theory of gravitation may destroy and simplify our web of fancy”. “Destroy and simplify” seem to cancel out, but however apt this elaborate metaphor (or simile) may be the new light of science has not to date shown much more than the older lamp of metaphysics.

Three hundred years of scientific advance since the Renaissance have led to great diversity of opinion rather than to a closer common understanding of the nature of mind-brain relationship. The erudition of the literature may impress, but, to borrow words of George Cloyne in another context in *The Times* newspaper (35), “it is a disagreeable thought that however deeply we plunge into the literature of the world—let alone its art and its music—we are unlikely to transcend our own experience of life”.

Scientific advance has been accompanied by shrinkage of perspective owing to specialization and the value of literature is lessened by volume and by the time taken up with sorting out the worthwhile from the trivial.

If, after all this work and speculation, we are no nearer to essential truth, it is fair to ask what is worthwhile and how much of what is written is repetition of the same theme in different words—merely tautology? Does it bring us closer to or take us further from

“a sense sublime

Of something far more deeply interfused,
Whose dwelling is the light of setting suns
And the round ocean, and the living air,
And the blue sky, and in the mind of man:
A motion and a spirit that impels
All thinking things, all objects of all thoughts,
And rolls through all things.” (36).

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SCHIZOPHRENIA—A HUNDRED YEARS AGO AND TODAY

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HAVE there been changes in the clinical picture of schizophrenic illness over the past hundred years? Or, to put the question more precisely, are the schizophrenic patients of the 1950s any different phenomenologically from their counterparts in the 1850s?

Recent studies (4, 5, 9, 14, 20) have aimed at determining the presence or absence of an increased incidence of psychosis in the nineteenth and twentieth centuries. Opinion is evenly divided as to whether the social changes of the twentieth century have significantly altered the epidemiology of psychotic illness. In spite of an increasing preoccupation with social psychiatry (13), the psychiatric literature is silent on the vital question "Have the changes of the past hundred years altered the clinical picture of schizophrenic illness itself?"

The present study, comparing a selected group of schizophrenics of both sexes from the 1850s and 1950s, begins the search for information on the change or lack of change in this important condition over the past hundred years.

MATERIAL

The material for this study is derived from the records of the Bethlem Royal Hospital. Detailed records are intact for all admissions from 1816 to the present time.

The records of 1853–1862 were selected for comparison with the 1950–1960 records of the present hospital. Originally we planned to use the 1850–1860 Bethlem records. In 1853, however, there was a change in the clinical staff of the hospital leading to the appointment of the conscientious reformer, Sir William Charles Hood, as resident physician (17). The records compiled during his tenure (1853–1862) are superior in quality and detail to those of his predecessors, and were found more valuable for our study. Since 1948 Bethlem Royal Hospital has been associated with the Institute of Psychiatry, and records have been compiled in the psychobiological tradition for teaching purposes.

METHOD

The initial presenting problem for the study was the proper selection of comparable material from past and present. The concept of schizophrenia, unknown to our predecessors, was not introduced into psychiatric nosology until the publication of Bleuler's famous monograph (2) in 1911. To determine the diagnostic criteria in vogue in the mid-nineteenth century we reviewed the psychiatric textbooks (3, 7, 10, 11, 15) used by the psychiatrists of the period. The generally accepted division of insanity at the time, and the one used at

Bethlem, was into Mania and Melancholia. Mania included the types of insanity with "confusion of ideas" (10, 15) and encompassed all syndromes where thought disorder was evident together with "violent passions" (10). Melancholia was diagnosed according to the presence of "depressing passions" with or without the concomitant presence of confusion of ideas.

It should be noted that admissions to Bethlem Hospital were usually acute cases, since according to the rules in force in the 1850s, no patient ill for more than twelve months was admissible. The following selection criteria were used for obtaining a comparable group of schizophrenic patients from the 1853–1862 records.

1. All patients over forty were excluded. Taking into account the current generally accepted age distribution of the various psychiatric syndromes, this selection factor tends to decrease contamination from Manic-Depressives, Involutionals, undetected chronic alcoholics, and undetected General Paretics.

2. All patients diagnosed as Melancholia, and all those diagnosed as puerperal psychosis were excluded.

3. All patients with sensorial or memory impairment were excluded.

4. From the remaining patients a group of 100 male and 100 female "schizophrenics" were selected. These patients all had:

- (a) Evidence of thought disorder—i.e. a notation that the patient's language was disconnected and incoherent (loosening of associations).

- (b) Evidence of secondary symptoms—i.e. delusions or hallucinations.

One hundred male and one hundred female schizophrenics were also selected from the 1950–1960 records. In order to make this group comparable, no patients over forty were selected. All patients in this otherwise randomly selected group also possessed evidence of thought disorder and delusions or hallucinations.

The following data were recorded:

1. *Age*.

2. *Marital status*.

3. *Religion*.

4. *Exciting and/or predisposing cause*. Statements concerning causation are included in the nineteenth century records and are of unusual historical interest.

5. *Heredity*. The presence or absence of mental illness in the patient's family was always recorded in both nineteenth and twentieth century records.

6. *Religious pre-occupations*—recorded as present if religion was the predominant concern of the patient's verbalizations during the illness.

7. *Sexual pre-occupations*—recorded as present if sexuality was the predominant concern of the patient's verbalizations during the illness.

8. *Length of stay in the hospital (weeks)*. It was a Bethlem rule that patients who failed to recover within a year had to be discharged from the curable wards of the hospital; these patients were often accommodated elsewhere until they could be admitted to the incurable wards.

9. Was the patient acutely disturbed? Estimation of psychomotor agitation made by the resident physicians.

10. Recording of treatment used in the 1850s, where given (but in less than half the cases is there any mention of treatment).

RESULTS AND DISCUSSION

Table I compares the data obtained from the mid-nineteenth and twentieth century schizophrenics. Table II summarizes data on the postulated exciting causes of the schizophrenic illnesses of the nineteenth century patients.

TABLE I

	Male 1853-1862	Male 1950-1960	Female 1853-1862	Female 1950-1960
Religious pre-occupations ..	60%	21%	48%	18%
Sexual pre-occupations ..	22%	38%	19%	45%
Both religious and sexual pre- occupations	11%	4%	11%	3%
Acutely disturbed	57%	9%	63%	7%
History of mental illness in the family	31%	24%	32%	33%
Marital status:				
Married	23%	19%	32%	38%
Single	77%	81%	68%	62%
Average age (years)	27.2	28.1	27.6	28.6
Length of stay (weeks) ..	37	17.9-18	35.7	21.5
Religion:				
Protestant	98%	83%	96%	84%
Roman Catholic	2%	13%	4%	10%
Jewish	1%	4%	—	6%
None	1%	—	—	—
Discharged:				
Cured	57%	—	57%	—
Uncured	39%	—	40%	—
Died	4%	—	3%	—

1. *Average age.* It is of interest to note that the average age of randomly selected male and female acute schizophrenics is essentially the same in the nineteenth and twentieth century groups. Thus, in the past one hundred years, there appears to have been little change in the decade of life when this illness is most likely to occur.

2. *Marital Status.* There appears to have been little alteration in the recorded marital status of male and female acute schizophrenics in the nineteenth and twentieth century groups.

3. *Religion.* The groups are comparable with respect to religion, with the exception that the twentieth century group contains a greater percentage of Catholics and Jews. This difference can be due to the relatively smaller number of Catholics and Jews in the population at that time. Wickham (19) states that only the Roman Catholic Church has shown a relative and absolute increase in membership since the mid-nineteenth century. The Jewish migration to London occurred after the mid-nineteenth century (8), accounting for the greatly increased Jewish population and the greater likelihood of having Jewish patients in the twentieth century population of the hospital.

4. *History of mental illness in the family.* The familial incidence of mental illness in the two groups is remarkably similar, except for a slightly lesser percentage in the male schizophrenics of 1950-1960. This data includes a history

of any form of mental illness in the patient's family, and has no reference to any particular psychiatric syndrome.

5. *Acutely disturbed.* Six times as many males, and nine times as many female schizophrenics were acutely disturbed in the nineteenth century compared with the twentieth century group. The data on the twentieth century group records admission behaviour, usually unmodified by tranquillization.

It is difficult to determine the significance of this finding. O'Donoghue (17) describes the changes that occurred in the patient clientele of Bethlem in the 1850s. One of the reforms (1856–1859) instituted by Sir William Charles Hood was to raise by degrees the social status of the patients eligible for admission. In 1859 the Board of Governors defeated an effort made by the Common Council to reserve the hospital for the pauper insane. During the tenure of Dr. Hood the physical format of the hospital was humanized, with the installation of sofas, carpets, pictures, etc. Iron guards were removed from the windows. From our modern concepts, these improvements should have exerted a soothing influence on psychomotor agitation.

Some patients were admitted to Bethlem after a short period of confinement at other private hospitals. Whether these other hospitals transferred mainly unmanageable patients it is impossible to say.

However, the tremendous discrepancies noted in comparing the nineteenth and twentieth century figures strongly suggest that psychomotor agitation was more prevalent in the nineteenth century male and female schizophrenics. The individual nineteenth century records present startling descriptions of violent behaviour seen with great rarity in our contemporary experience. One of us (J.G.H.) has been impressed with the decrease in psychomotor agitation in the schizophrenic population during his twenty-five years at Bethlem.

6. *Length of stay.* Before discussing the data on length of stay we must mention a cut-off factor used in tabulating the 1853–1862 results. Although length of stay at Bethlem during the mid-nineteenth century was officially limited to one year, some patients, for special reasons, remained for one-and-a-half to two years. In order to avoid distortion of the results by these few patients, all such patients were tabulated as having been hospitalized for sixty weeks. The same factor was used for the 1950–1960 group.

A comparison of the results shows that the nineteenth century patients remained in the hospital approximately twice as long as the twentieth century patients. It was a hospital practice in the nineteenth century to grant patients a month's leave of absence prior to discharge. If the patient functioned well during the month away from hospital, he was seen again by the Committee and discharged. Our figures are based on the period of hospitalization prior to the granting of the leave of absence.

Why were patients hospitalized longer in the mid-nineteenth century than today? Only speculations can be given in answer to this question. First, it appears from the data on psychomotor agitation that the mid-nineteenth century patients were more acutely disturbed than the twentieth century schizophrenics. These patients may have been more seriously ill, and hence stayed longer in hospital. Secondly, little treatment was available in the 1850s. Sedation was attempted by using tincture of morphine and was never very effective. Although the patients were closely observed by medical personnel while in hospital, there was an absence of any after-care facilities, possibly necessitating longer hospitalizations. In the 1950s, with an excellent follow-up clinic and a Day Hospital available, patients could be discharged and yet remain under close supervision.

7. *Discharged, cured or uncured.* Each record in the 1853-1862 group contains a statement that the patient was cured or uncured on discharge. The cured percentages of male and female schizophrenic groups during this period were exactly the same. The uncured and death percentages were also nearly equal.

In the 1850s, one of the main duties of the mental hospital was to protect the public from destructive acts perpetrated by those with mental disturbance. Therefore, although criteria of cure are not stated, it appeared to us that freedom from the desire to injure the self or others was the main standard used by the discharging committee. The statistics seem to indicate the improvement and abatement of acute disturbance, rather than disappearance of primary and secondary symptoms in the modern sense.

8. *Religious preoccupations.* The most striking data obtained from the study concerns the tremendous preponderance of religious preoccupations in the mid-nineteenth century schizophrenics. Three times as many male and female acute schizophrenics had religious preoccupations in the nineteenth century as compared with the twentieth century group.

We feel that this finding has great import for contemporary phenomenological research in schizophrenia. Bleuler embraced the early work of Freud and Jung, and encouraged attempts to comprehend the causation and clinical picture of schizophrenic illness through a careful analysis of the symptoms. Buttressed by the support of so eminent a clinician as Bleuler, contemporary psychoanalysts have delved deeply into the meaning of schizophrenic symptomatology, thinking that such detailed investigation would aid in unravelling the cause and nature of the illness. Since the observations of Freud, psychoanalysts have emphasized the importance of sexual disturbance in the aetiology of mental illness, including schizophrenia. If these psychoanalysts had been considering patient material of the mid-nineteenth century, they would have been forced to consider religious disturbance as a prime factor in the aetiology of schizophrenia.

Indeed, some psychiatrists of the period did consider religious excitation (3, 7, 15) a cause of insanity. However, an examination of the position of religion in mid-nineteenth century England shows that the religious preoccupations vented by schizophrenic patients were largely culturally determined.

Wood (21) contrasts the position of religion in England in 1850 with that of 1950. He states "a hundred years ago, organized Christianity, including in the term both the Established Church and the Free Churches, played a greater part in the life and thought of Britain than it does today. Today not more than 10 per cent. of the nation joins in public worship" (Rowntree's data from York). Addison (1) and Wickham (19) provide us with objective religious data in 1851 England. Although questions regarding religious affiliation were not permitted during the official census, Horace Mann, a barrister, was asked by the Registrar General to organize a voluntary count of all people worshipping on a certain Sunday throughout England and Wales. The day chosen was 30 March, 1851. Out of a total census population (England and Wales) of 17,927,609 and church sittings of 10,212,563, the actual attendance at all churches was 7,261,032, or over 40 per cent. of the total population.

Wood (21) says "A hundred years ago, the Puritan tradition, with its sharp division between the Christian way of life and the life of the world, with its distrust of what we should now regard as innocent pleasures, with its emphasis on self-discipline and self-control, powerfully influenced all classes and churches. Allied to the Puritan interpretation of what it means to keep oneself unspotted

from the world was the idea of the world as a vale of tears, and of the Christian as a pilgrim journeying to a better land".

How religion permeated life in the early Victorian age is described by Kellett (12), "In the family the father ruled by divine right; the mother, however dexterously she might continue to be the real power behind the throne, openly supported his authority, and the children were kept in subjection." . . . "It was constantly emphasized that the will of the parent was the Will of God. and that disturbance of the parental convenience was a sin of the first magnitude. The more religious a father was, the more likely was he to make this confusion between his own inclinations and the divine purpose."

Education during this period (until 1870) was under religious control (1, 18). Schools were maintained by the Established Church and by the Non-conformists. Moorman (16) tells us "At most of these schools (public and grammar) there was a strong Church atmosphere. The headmaster was often in Holy Orders, as were several members of the staff. The Chapel played an important part in the life of the school. Confirmation was administered almost as a matter of course, and many of the boys were taught to read the New Testament in Greek." Kellett (12) tells us how children were frightened by pictures of Judgement Day, and by gruesome descriptions of the torments of Hell. Even books for pre-school children had chapters on Hell. God was portrayed as a tyrant whose eyes were in every place, watching for delinquency.

Small wonder then, that the schizophrenics of the period were preoccupied with religion, when nearly every other member of the society was. It does not seem strange that a mid-nineteenth century adult, having been trained to flee the fires of Hell since childhood, should hear God's voice and fear His retribution after developing a schizophrenic illness. Or, to draw an analogy, that may or may not be far-fetched, the mid-twentieth century adult, exposed to our society's preoccupation with sex, may be expected to develop sexual pre-occupations during a schizophrenic illness. We wish to point out that such pre-occupations may be culturally determined. Looking for the aetiology of schizophrenic illness in a detailed analysis of religious and/or sexual delusions or hallucinations appears to be a futile search.

9. *Sexual preoccupations.* Approximately twice as many male and female acute schizophrenics had sexual preoccupations in the twentieth century group as compared with the nineteenth century group.

What this comparison means is difficult to determine. We have no even faintly reliable data concerning sexual behaviour and attitudes of mid-nineteenth century England except the oft-mentioned general impression that it was a period of sexual suppression. If this is a valid assumption, we would expect outpourings of sexual material during acute schizophrenic illness, according to Freudian theory. Actually, twice as many twentieth century schizophrenics had sexual preoccupations as did nineteenth century schizophrenics. If sexual factors are of vital importance in schizophrenic illness, they should have been just as important and prominent in the 1850s as they are in the 1950s.

One objection will doubtlessly be raised against our data: it will be claimed that the psychiatric examiners of the period were products of their age of suppression and did not record sexual data where it was prominent. Our perusal of the mid-nineteenth century records revealed that this was not the case. Florid sexual material vented by both male and female patients was faithfully recorded. With female patients, sexual factors connected with menstruation and pregnancy were carefully observed. Delusions of infidelity and delusions

regarding homosexuality were recorded where present. The only sexual sphere where wild speculation was found occurred in reference to male masturbation, which was still believed to be a cause of insanity. Statements like "The patient looks as though he masturbates, but he has not yet been observed doing it" and "His insanity is obviously due to masturbation" were frequently found in the nineteenth century records.

10. *Both sexual and religious preoccupations.* Both sexual and religious preoccupations were found present in approximately three times as many male and female patients in the nineteenth century as in the twentieth century group. Interestingly enough, we have observed that three times as many male and female schizophrenics had religious preoccupations in the nineteenth century as compared with the twentieth century group. The greater number of nineteenth century patients having both sexual and religious preoccupations is most likely due to the greater number of patients having religious preoccupations during that period.

11. *Miscellaneous.* Postulated Causation and Treatment of Schizophrenic Illness during the mid-nineteenth century. In the mid-nineteenth century records speculation regarding causation was recorded under two headings, "exciting cause" and "predisposing cause". The data presented in Table II is a compendium of the "exciting causes". "Predisposing causes" were usually

TABLE II
"Exciting Causes"

Exciting Cause 1854-63						Male Per cent.	Female Per cent.
1. Sexual factors (love, masturbation, etc.)	..	..	..	..	..	7	20
2. Masturbation	..	..	..	..	..	11	1
3. Employment and business difficulties	..	..	..	..	..	12	6
4. Religious excitement	..	..	..	..	..	11	8
5. Overwork	..	..	..	..	..	13	5
6. Anxiety	..	..	..	..	..	7	7
7. Death of a loved one	..	..	..	..	..	5	9
8. Politics	..	..	..	..	..	2	0
9. Not known	..	..	..	..	..	27	34
10. Others	..	..	..	..	..	5	10

"hereditary" or "previous attack". Most of the speculations about causation represent an attempt to establish a temporal relationship between events in the patient's life and the onset of the illness.

The *treatment* prescribed for these mid-nineteenth century schizophrenics is also of unusual historical interest. As previously stated, in less than half the cases was there any record of the treatment prescribed. The following is a list of the medicaments mentioned, alas, all too briefly:

Liq. Morph.	Ol. Tighi	Antim. Pot. Tart.
Mist. Camph.	Mag. Sulph.	Castor Oil.
Decoc. Alves	Spt. Ammon.	Cinchona.

O'Donoghue (p. 363) reported that two padded rooms were installed in Bethlem in 1844 for the safety of disturbed patients.

No references to the use of cupping at this time were found and in only two of the two hundred cases was bleeding recorded as being used for treatment.

Narcotics were largely prescribed, but Haslam (10) had earlier stated that opium given to patients with violent paroxysms hardly ever produced sleep and might render the patient more furious. Burrows (3) later believed that

opium given in the early stages of the illness induced sleep by "increasing sanguinous congestion and compressing the brain".

Purging and Vomiting. Haslam (10) had stated that diarrhoea often produces a natural cure of insanity. Burrows (3) condemned purging but also said that "keeping the bowels in free action is indispensable in all cases of insanity, but absolute purging in the incipient and active stages is especially necessary". He agreed that there is no remedy for the cure of insanity that is more generally or strongly recommended than vomiting.

Camphor. Haslam (10) says of camphor that "this remedy has been highly extolled, and doubtless with reason".

Burrows (3) says of it that "the anti-maniacal virtues of this substance have been highly extolled and some marvellous cures ascribed to it" but he added "modern physicians have little confidence in it".

The *London Medical Journal* (Vol. VI, p. 120, about 1820) reports a case where a "perfect cure" followed a camphor-induced fit in a case of mania. This is of particular interest in view of the use, more than a century later, of convulsion treatment in the psychoses, and of camphor as the convulsant drug. But the final conclusion in the *Journal* was that "in rash hands it may prove dangerous to life and in the most dexterous can rarely be used to advantage".

Tube Feeding. Burrows (1928) after referring to the number of patients with delusions of being poisoned who were refusing food, says, "I have seen liquid nutriment introduced into the stomach most readily by means of a stomach pump; passing its hollow elastic tube through a nostril is more readily made than forcibly separating the jaws."

G. W. Daniels, in his article on Forced Alimentation, in *The Journal of Mental Science*, 1856, Vol. 2, p. 121, says that after many years of practical experience, he doubted the propriety of its use in all cases where the patient is refusing food, "though it is sometimes successful". He described the death of three patients in spite of this method of treatment.

Koch in *The Journal of Mental Science*, 1871, Vol. 16, p. 624, describes a "new feeding apparatus for the insane".

SUMMARY

In order to answer the question "Have there been changes in the clinical picture of schizophrenic illness over the past hundred years?" a selected group of schizophrenics of both sexes from the 1850s and 1950s is compared. Methods of proper selection of comparable material from past and present are described. Data are presented in two tables.

Comparing the two groups revealed:

1. The average age was essentially the same in the nineteenth and twentieth century groups.
2. The proportions of married and single patients was almost the same in both groups.
3. Both groups were comparable with respect to religion. Differences were related to factors unconnected with schizophrenic illness.
4. Remarkably similar familial incidence of mental illness in the nineteenth and twentieth century groups was found.
5. Six times as many male and nine times as many female schizophrenics were acutely disturbed in the nineteenth, as compared with the twentieth century group. This discrepancy was discussed. It was thought probable that psychomotor agitation was more prevalent in the nineteenth century schizophrenics.

6. Length of hospitalization was twice as great in the nineteenth as in the twentieth century schizophrenic group. Reasons for this were discussed.
7. The cured percentages of the nineteenth century male and female schizophrenic groups were exactly the same. What "cured" meant in the mid-nineteenth century was discussed.
Three times as many patients of both sexes had religious preoccupations in the nineteenth century as compared with the twentieth century group. This was the most striking finding of the study, and its importance for contemporary phenomenological research in schizophrenia was emphasized.
8. Summarizing the position of religion in mid-nineteenth century England, it was shown that religious preoccupations were largely culturally determined. It was suggested that the contemporary sexual preoccupation of schizophrenics might also be culturally determined. Delving deeply into the meaning of symptoms that might be culturally determined was not thought helpful in searching for the cause of schizophrenic illness.
9. Approximately twice as many patients of both sexes had sexual preoccupations in the twentieth century group as compared with the nineteenth century group. Reasons for this difference were discussed.
10. Approximately three times as many patients of both sexes had both sexual and religious preoccupations in the nineteenth century as in the twentieth century group.
11. Data was presented on the postulated causation and on the treatment of schizophrenic illness during the mid-nineteenth century.

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A NEUROPHYSIOLOGICAL THEORY OF SCHIZOPHRENIA*

By

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IN the psychiatric literature in English there is a tendency today to deprecate speculative neurophysiological theories of the "functional" psychiatric disorders, while little is done to halt the flood of far-fetched and untestable hypotheses of the "dynamic" psychopathologists. The present author believes that it is legitimate to postulate a neurophysiological basis for schizophrenia in the hope that those with a more adequate knowledge of neurophysiology will be obliged to re-examine the problem of schizophrenia more fruitfully. Conrad (1958, and Fish, 1960) has given a very interesting interpretation of acute schizophrenic symptoms, so that his approach to schizophrenia forms a very convenient starting point for further speculation. This worker's views will therefore now be presented in some detail before the present author's theories are discussed.

A.—THE GESTALT THEORY OF SCHIZOPHRENIA

Using Gestalt theory, Conrad has put forward the view that in schizophrenia there is a loosening of the coherence of perception and thought which results in the emergence of new gestalts† and the fragmentation of psychic activity. He divides the acute schizophrenic shift into five phases, viz.:

1. The Trema.
2. The Apophanous Phase.
3. The Apocalyptic Phase.
4. The Consolidating Phase.
5. The Residual Phase.

We will now consider each of these in turn.

1. *The Trema*

This word comes from the German stage slang for the stage fright which is experienced just before the actor makes his entrance. In this phase the patient feels that he has lost his freedom of action, is somehow shut in and unable to communicate with his environment. Often there is marked anxiety, but depression is also common and usually associated with ideas of guilt and disgust with life. Frequently during the trema the patient carries out rather silly actions which he rationalizes, but which are out of keeping with his personality and the total situation. These senseless actions may lead the patient into difficulties which may then be incorrectly understood as reactive factors in the production of the depressive mood or the schizophrenic symptoms. In many patients there is a general feeling of suspicion which pervades all experiences and leads to the feeling that there is "something going on". In the end a delusional mood occurs in which the environment is experienced as being changed in a strange and

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† In order to avoid confusion, the word "gestalt" will be treated as an English technical word, so that the plural form is gestalts, not gestalten, which is the German plural.

threatening way. Conrad believes that there is scarcely a case of early schizophrenia in which this phenomenon does not occur. The delusional mood marks the point of transition from the trema to the apophanous phase. The trema itself may last for days, weeks or months and may even subside before the next phase can occur.

2. *The Apophanous Phase*

Delusional experiences in which a delusional significance occurs in connection with a psychological event such as a perception or a sudden idea have been considered by most German workers to be the primary irreducible elements of schizophrenic delusions. Since the German technical terms for these phenomena are cumbersome, Conrad has introduced the term apophany to include all these delusional experiences. This word, which is Greek for "becoming manifest", is a very suitable term for these strange experiences, in which a new meaning becomes manifest in connection with a psychological event. This word also has the advantage that the adjective apophanous can be derived from it, so that it is possible to refer to apophanous ideas, moods, perceptions and so on. Conrad has therefore called the phase in the schizophrenic shift in which delusional experiences occur the apophanous phase.

Before we consider the apophanous phase in detail, we must discuss delusional perception, since this was the first schizophrenic symptom which was subjected to Gestalt analysis (Matussek, 1952, 1953). Delusional perception occurs when an abnormal significance, usually in the sense of self-reference, is attributed to a normal perception in the absence of any rational or emotional reason. The older German investigators all insisted that delusional perception was not due to a disorder of perception itself, but a disorder of thought. However, recently Matussek (1952, 1953) has pointed out that there are two varieties of delusional perception, one in which perception is disordered and the other where verbal associations are important. An example of this second variety is a patient of mine who heard a floorboard squeak as a colleague stood on it and looked down to see the linoleum. The word "Lino" came into his mind and was followed by the thought "No lie", which he took as an instruction from his colleague that he was not to lie. This resembles the play upon words of the obsessional ruminator who repeatedly finds indications of his obsessional thoughts in his environment. In the other variety of delusional perception, Matussek believes that there is a loosening of the coherence of perception which allows the essential properties of the object to come into undue prominence.

This concept of the essential property comes from Metzger (1954) who considers that percepts may have three types of gestalt property, structural properties, total quality properties and essential properties. Structural properties are those of arrangement and organization, such as figural form, brightness, colour profile and so on. The properties straight, round, closed and constant are examples of structural properties. The total quality properties are material properties which are not simple sensory qualities independent of structure. The properties indicated by the adjectives transparent, rough and soft are examples of total quality properties. Essential properties are expressions of the essence of the object and include all the physiognomic or expressive properties such as character, ethos, habitus, mood, emotional value and so on. Solemn, friendly, proud and elegant are examples of these properties. Although they have been called "subjective impressive qualities" or subjective impressions, Metzger insists that they are perceptually given properties which immediately make an impression on us and affect our own essence.

The apophanous phase can be divided into two subphases, apophany of external space and apophany of internal space. In the first, all external events which are experienced acquire a new significance, while in the latter internal psychic events acquire a special meaning. One can use a more familiar jargon and talk of apophany of perception and apophany of the mediating processes.

Delusional perception is the most common definite external apophanous experience and Conrad has differentiated three stages of this phenomenon, viz.:

Stage 1. The perceived object indicates to the patient that it concerns him, but he cannot say to what extent. This is pure apophany.

Stage 2. The perceived object indicates to the patient that it concerns him and he knows the extent of this immediately. Thus, for example, he may know that it has been put there to test whether he observes it. Thus the patient has the experience that the object or sequence of events has been made for him, or, in other words, he has prefabricated experiences.

Stage 3. The perceived object signifies something quite definite, and the essential properties of the percept have come into prominence because of a change in the total structure of perception. This is delusional perception in the strict sense of the word.

This undue prominence of essential properties explains the misidentification of persons, which is common in the apophanous phase. Unknown persons may be recognized as friends or acquaintances, while relatives and friends are not recognized as such by the patient. The emerging essential properties can be considered as causing these confusions of identity.

The patient is, of course, in the ptolemaic position in that he experiences himself at the centre of things. This being so, he may attribute a delusional significance to his actions and have the experience of omnipotence. Conrad calls the experience of being the centre of the world "anastrophe" and regards it as the subjective aspect of apophany or, as he puts it, "Whenever there is apophanous experience, the ego must, at the same time, be anastrophically changed."

From Conrad's case material it seems as if there is a barrier which stops the schizophrenic process from passing quickly from the perceptual field to the mediating processes. In some cases apophany only affected external space, while in others it was some time before internal space was affected. Once internal space is affected, there is a loosening of the coherence of the mediating processes. Memory images may lose their connection with the total field and be experienced as delusional inspirations. Thought broadcasting in which the patient's thoughts become manifest to the environment can be regarded as the reverse of delusional perception, where a new significance of a perception becomes manifest to the patient. The loss of adequate figure ground relationships in conceptual thinking naturally leads to the patient hearing his own thoughts spoken aloud, so-called Gedankenlautwerden. As the disorder becomes worse, all personal indication of the thoughts is lost and hallucinatory voices occur. Bodily hallucinations can be understood as the effect of apophany on bodily sensations and the body image.

3. *The Apocalyptic Phase*

If the schizophrenic process is very severe, the loosening of the coherence of perception and of the mediating processes may lead to fragmentation of psychic life or the apocalyptic phase. This is another name for catatonia, since the release of the representations of bodily sensations and body movements

leads to a gross motor disorder. Sense continuity is destroyed, and this accounts for the fact that frequently only fragments of the total experience can be remembered after an acute catatonic illness. Rarely the gross fragmentation of psychological activity in the apocalyptic phase leads to death. This is the acute, deadly catatonia of Stauder.

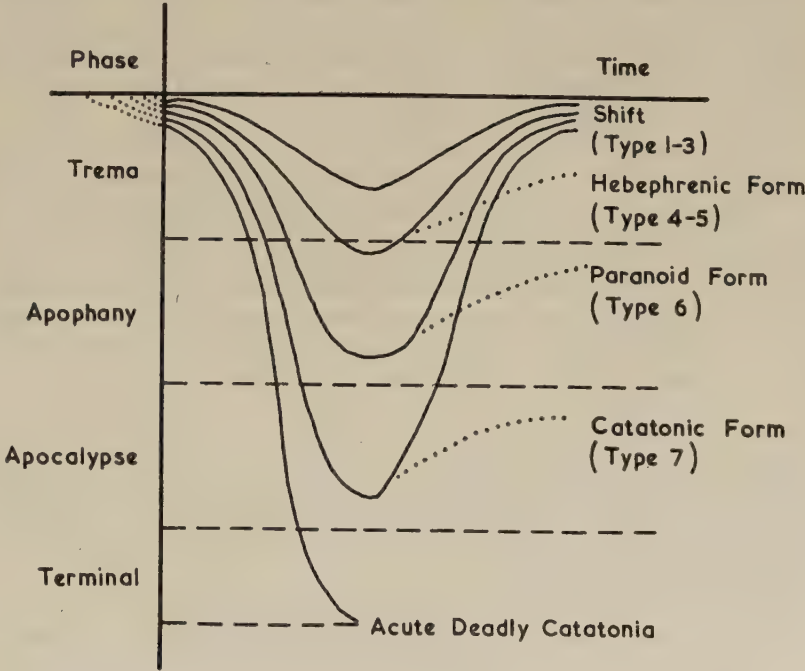
4 and 5. *The Consolidatory and Residual Phases*

The phase of consolidation begins after a few weeks or months, and a final residual phase occurs. In this phase there may be no active symptoms, but the patient feels less capable intellectually than before the shift, but this change is often more subjective than objective. Conrad suggests that every individual has his own energy potential or his particular ability to direct and apply his energies and that this potential is lower than normal in the residual phase.

Conrad has described seven different types of course of illness in a schizophrenic shift. These are shown in Figure 1. They are:

Type 1. The process does not pass beyond the trema, only abuts on the apophanous phase and usually subsides in a few weeks. The loss of energy potential is minimal, so that the defect is very slight.

Type 2. The process passes through the trema, enters the apophanous phase and subsides after a few weeks, leaving behind only a slight loss of energy potential.



VARIETIES OF ACUTE SCHIZOPHRENIC SHIFTS
(AFTER CONRAD)

FIG. 1.

Type 3. The trema and apophanous phases are quickly passed through, and the apocalyptic phase is briefly touched on before the process subsides: the residual defect is very slight.

Type 4. The trema and the apophanous phase are rapidly passed through without the psychosis being recognized, but a marked loss of energy potential occurs, so that the residual state may make adaptation difficult. This corresponds to the type of schizophrenia called dementia simplex by some authors, but it can also be regarded as a variety of hebephrenia (Fish, 1957).

Type 5. The process does not pass beyond the trema, but there is a severe loss of energy potential. This is another way in which a chronic hebephrenic illness can occur.

Type 6. The process reaches the apophanous phase and is arrested there, so that a chronic paranoid schizophrenia occurs.

Type 7. The process reaches the apocalyptic phase and is arrested there, giving rise to a chronic catatonic clinical picture.

Conrad's theory links together all the phenomena which occur in acute schizophrenia by supposing that they are the result of a loosening of the coherence of psychological activity produced by a neurophysiological disorder. His concept of loss of energy potential in the defect state has nothing to do with Gestalt theory and is merely another term to explain the peculiar psychological disability in this condition. It is difficult to understand why Conrad has not attempted a Gestalt explanation of the defect states. Is it not possible, for example, that the so-called loss of energy potential can be explained as a partial loss of the normal integration of psychological functions or the mediating processes, while the paranoid and catatonic syndromes are abnormal re-integrations of the mediating processes?

This failure to deal with the chronic schizophrenic syndromes can be considered as a product of Gestalt theory itself which dogmatically insists that all psychological phenomena occur in figure ground relationships which are not learned, but innate and immutable. If this is so, then once the interfering schizophrenic neurophysiological process subsides, the previous figure ground relationships should reassert themselves. This would lead to the conclusion that in the paranoid and catatonic chronic clinical pictures the process was still active. However, the classical apophanous experiences do not occur in chronic paranoid schizophrenia.

B.—A NEUROPHYSIOLOGICAL THEORY OF SCHIZOPHRENIA

There is no doubt that Conrad's views are extremely stimulating, but as they stand it is difficult to use them as in a heuristic way. If we re-interpret his ideas in terms of D. O. Hebb's neuropsychological theories, I believe we have a much more useful set of ideas.

Hebb (1949) has summarized his basic theory as follows:

"Any frequently repeated particular stimulation will lead to a slow development of a 'cell assembly', a diffuse structure comprising of cells in the cortex and diencephalon (and also perhaps in the basal ganglia of the cerebrum), capable of acting briefly as a closed system, delivering facilitation to other such systems and usually having a specific motor facilitation. A series of such events constitutes a 'phase sequence'—the thought process. Each assembly action may be aroused by a preceding assembly, by a sensory event or—normally—by both. The central facilitation from one of these activities on the next is the

prototype of 'attention'. The theory proposes that in this central facilitation, and its varied relationship to sensory processes, lies the answer to an issue that is made inescapable by Humphrey's (1940) penetrating review of the problem of the direction of thought.

"The kind of cortical organization discussed in the preceding paragraph is what is regarded as essential to adult waking behaviour. It is proposed also that there is an alternate 'intrinsic' organization occurring in sleep and in infancy which consists of hypersynchrony in the firing of cortical cells. But besides these two forms of cortical organization there may be disorganization. It is assumed that the assembly depends completely on a very delicate timing which might be disturbed by metabolic changes as well as by sensory events which do not accord with the pre-existent central process. When this is transient, it is called emotional disturbance; when chronic, neurosis or psychosis."

Hebb's views can be partly represented diagrammatically as shown in Figure 2, which is a modification of a diagram by Hebb (1958). C is the central

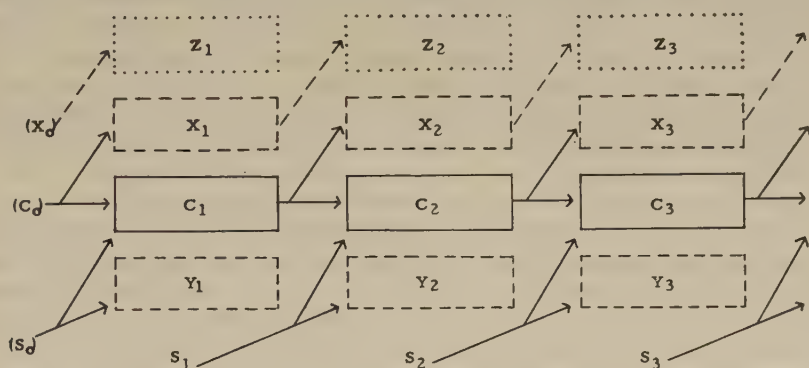


DIAGRAM OF THE SELECTIVE PROCESS IN THINKING (AFTER HEBB)

FIG. 2.

process consisting of simultaneous active assemblies at three successive moments, while S is the corresponding sensory input. X is an assembly subliminally excited by the central process, Y is an assembly subliminally excited by the sensory input, and Z is an assembly which would become subliminally excited if X were excited. In the following discussion these assemblies will be referred to as X, Y and Z assemblies. C consists of assemblies which are active because they receive excitation from both sources, but if one accepts Milner's modification of the cell assembly (Milner, 1957), then priming of X assemblies may lead to the central process deviating to X assemblies and to the sensory input having little influence on the central process for a short time. This diagram does not show the effect of the arousal or reticular system, but this system is, of course, partly responsible for the maintenance of the central process, since it facilitates the firing of cortical neurones.

Let us suppose that in schizophrenia there is an overactivity of the reticular system. This would lead to the X and Y assemblies being nearer their firing thresholds than usual. It could be argued that the Y assemblies would be more likely to fire than the X when the reticular overactivity is relatively mild, since the sensory input would tip the balance. It may also be that the sensory input at the cortical level is enhanced by reticular overactivity, although there is evidence that it is diminished at lowest levels by reticular stimulation.

Fuster de Carulla (Magoun, 1958) has shown that briefly presented visual clues are perceived better during reticular stimulation, while Lindsley and Griffith (Magoun, 1958) have found that the recovery cycle of the optic cortex was shortened by reticular stimulation. Thus it seems reasonable to assume that sensory events would, under these circumstances, acquire a much greater significance and would tend to produce disorganization of the phase sequence much more often than usual. Such a disorganization would, of course, express itself as anxiety. This anxiety, together with the increased significance of neutral events, would account for the general feeling of unease and suspicion which is so characteristic of the *trema*. This diversion of the central process would initially be sporadic and the central process would be mainly determined in the normal way by the previous cell assembly and the sensory input. However, as the intensity of the reticular overactivity increased, the central process would be diverted and disrupted by the sensory input much more frequently. When this disruption was occurring very often, then a delusional mood would occur. The uncanny, unpleasant nature of this symptom can be explained as due to the repeated disorganization of the central process by the sensory input and the feeling of some unknown significance produced by the increased impressiveness of all perceptions. In the end, some sensory event would dominate the central process for some time and the individual would experience a kind of fascination for this object. It would therefore acquire a new meaning for him, i.e. it would be a delusional perception.

Here we must pause to consider Matussek's concept of the coming into prominence of essential properties in delusional perception. Objects with marked essential properties have a greater tendency to evoke an emotional response, or, to use Hebb's terminology, to produce a cortical disorganization. Such objects would therefore be likely to form the subject of delusional perception. Since human beings have marked essential properties, the perception of others could lead to cortical disorganization and this could give rise to a lack of recognition, since the appropriate phase sequence would not be aroused, because another X or Z assembly takes over the train of thought. Conversely, a similarity of features may lead to a domination of the central process by the sensory input and the evocation of a memory image (phase sequence) of a relative.

However, once the reticular overactivity passed a certain point, sensory over-determination of the central process would not be the only expression of the disorder, since a central process, consisting entirely of Z cell assemblies, could then become active independent of the specific sensory input.

Any central process consisting of X or Z assemblies could either be the sole central process or a central process running parallel with, but interfering with the "normal" central process which is sensorily over-determined. Such a parallel process seems necessary in order to explain auditory hallucinations which are, of course, experienced as being foreign. It seems legitimate to assume that the sense of self is associated with the central process which is determined by the preceding phase sequences and the sensory input. Normally, the central process gives rise to behaviour leading to environmental modification, which, in turn, leads to a modification in the sensory input which then influences the central process and so on. Thus the interaction of the organism with the environment in which the self is experienced is expressed in the central process. In the *trema* the sensory over-determination of the central process gives rise to a paranoid attitude. The ability of the central process to detach itself from sensory input, but to be reorganized by impressive sensory input is lost. If a

parallel process were set up and constantly interfered with the central process, then these interferences would be experienced as foreign thoughts or voices, since the only continuous influence on the central process which has been previously experienced is a sensory one. Sudden ideas or "brain waves" can be considered as interference with the central process in normals, but they are, of course, brief and probably have a connection with the on-going central process, which is not conscious, but is expressed by the fact that the idea is not foreign. Since language plays such a great part in human thought, it is easy to understand that the interfering parallel process is often experienced as hallucinatory voices.

This parallel process need not be permanent, so that there could be periods when the sensorily over-determined central process alone was active, other periods when the parallel process was active on its own, and others when the two processes ran parallel. The phenomenon of hearing one's own thoughts spoken aloud could be understood as an activation of the phase assemblies for words which are activated to just below threshold by the reticular system and fully activated by the central process. They would thus occur just before the commencement of the parallel process.

This change from apophany of external space to apophany of internal space could be regarded as the point at which X assemblies become independently active. Thought insertion, and thought deprivation can be explained as due to the parallel process.

The acute catatonic phenomenon can be explained in one of two ways. Firstly, if the parallel process became entirely autonomous and the normal central process disappeared, then gross behavioural disorder would occur, since behaviour would not be influenced by sensory input. Some acute catatonic illnesses and oneirophrenia could be accounted for in this way. Again rapid switching between two parallel processes, an autonomous central process and a sensorily dominated central process could cause stupor, since neither central process could find motor expression before it was interrupted by the other. Finally, in young people, especially children, disordered thinking expresses itself as disordered behaviour. There is no doubt that catatonia is a disorder of the young. Thus X and Z assemblies in young people are much more likely to have a strong motor element, and it is reasonable to assume that maturity is associated with an increase in X assemblies which are not directly expressed in motor activity. Thus a parallel central process in a young person would be likely to cause non-goal-directed motor activity.

The maintenance of this parallel central process poses a question. Chance factors might lead to the establishment of such a process, but without continuous reinforcement it would tend to link up with the sensorily directed central process. It is possible that a parallel process might be kept going by reverberatory circuits involving the so-called cortical association areas and the subcortical nuclei. It has been shown that in the monkey there are corticofugal projections to the arousal system which are only second in importance to the inflow from the peripheral receptors (French, Hernández-Peón and Livingston, 1955). Segundo, Naquet and Buser (1955) found the cortical areas which were specially effective in producing arousal were the cingulate, orbital and lateral frontal regions, the para-central and para-occipital areas and, I would like to stress this, particularly the superior gyrus and pole of the temporal lobe. Supposing that this is also true of the human brain, then a parallel process taking place in one of these areas might produce even more reticular over-activity, but this would have no reinforcing effect on the sensorily dominated

central process, but would reinforce the parallel central process. It could well be that the ability to form a stable parallel process is the decisive factor in true schizophrenia. All the previous disorders are potentially reversible, but once the parallel central process is firmly established, the further progress of the illness is no longer dependent on the original reticular overactivity, but on this vicious circle which is now firmly established.

I think that it is possible to account for the symptoms of the different varieties of paranoid schizophrenia by postulating that the parallel process interferes to a varying degree with the normal central process and the parallel process involves different cortical areas.

I hope to deal with this problem in more detail in a later paper, and I would like now to consider the chronic schizophrenic states. If the reticular overactivity disappears before a stable parallel process has been established, then it is obvious that complete recovery could occur. The chronic states can be accounted for in three ways:

- (a) The reticular overactivity does not decline. This would account for those paranoid illnesses in which delusions remain affect laden for years and which Leonhard has called affect laden paraphrenia (Fish, 1958; Leonhard, 1959).
- (b) The reticular overactivity subsides, but the cortical vicious circle remains. This would account for many paranoid and catatonic clinical pictures with marked affective blunting.
- (c) The reticular overactivity subsides, but it has caused a cortical reorganization which is less efficient than the premorbid one. Hebb's theory is based on the postulate that some structural change occurs at synapses which are repeatedly active. In the sensorily dominated central process produced by reticular overactivity all those cell assemblies and phase sequences which were well linked to perception would be reinforced, while the ones mediating more abstract thought would not be. Thus if the reticular overactivity were severe enough or prolonged enough, a reorganization of phase sequences could occur which would make abstract thought more difficult. Thus a mild, prolonged tremor could cause a marked defect. This would account for the insidious development of schizophrenic defect in the eccentric and silly hebephrenias (Leonhard, 1959).

The course of the illness would obviously be determined by the duration and severity of the primary reticular overactivity and also by establishment of a stable parallel process. This theory would explain the atypical or schizo-affective psychoses as being due to a reticular overactivity without the formation of a stable central parallel process. It would also explain that strange condition paranoid depression in which there are marked persecutory delusions which go far beyond what can be understood as due to projected guilt feelings. These depressions are always associated with marked anxiety (Bumke, 1948; Janzarik, 1959) and attempts have been made in the past to explain the paranoid delusions psychologically as partly due to the severe anxiety. The theory presented here would suggest that the severe anxiety is associated with marked overactivity of the reticular system. This would produce a sensorily dominated central process which would lead to a paranoid attitude.

We must now come to the evidence in favour of reticular overactivity as the cause of schizophrenia. The most important supporting evidence is the fact that amphetamine overdosage in normal individuals will produce a psychosis which is clinically indistinguishable from paranoid schizophrenia. It is, of

course, well known that amphetamine produces an arousal response in the EEG in animals which is similar to that produced by reticular stimulation. Bradley and his associates (1957) have produced evidence to suggest that in animals amphetamine stimulates activity in the reticular system below the level of the midbrain. Now both sodium amylobarbitone and chlorpromazine will produce marked improvement in patients suffering from acute schizophrenic shifts. There is evidence (Bradley, 1957) that chlorpromazine may block the receptors related to afferent collaterals entering the reticular formation at brain stem and mesencephalic levels. Thus it is fair to assume that such general blocking of the afferents to the reticular system would diminish reticular activity. There is evidence that pentobarbitone and other quick-acting barbiturates depress activity within the reticular system. This would account for the effectiveness of sodium amytal in acute schizophrenic clinical pictures. The fact that it often has no effect on chronic patients would again support the theory presented here. One final point about amphetamine is that it has been shown to cause worsening of symptoms in schizophrenia.

An interesting clinical condition which supports our present theory is the paranoid psychosis in clear consciousness which occurs in some epileptics. Some of these patients show a relatively normal electroencephalogram when psychotic and an epileptic electroencephalogram when normal. Landolt (1958, 1960) has suggested that the normal electroencephalogram is a "forced normalization" produced by overactivity of the arousal system. This worker has also claimed to have found "forced normalization" in thirty-two non-catatonic schizophrenics. The electroencephalographic investigation of a series of fifty adolescent schizophrenics by Lescable, Lelord and Fardeau gives some support to the theory presented here. There was an immature record in 18 per cent. of their patients and in only one patient with such a record was the outcome unfavourable. In 74 per cent. the record was normal, but eighteen of the records in this group were rich in fast rhythms and five were hypermature. The outcome in these five patients was unfavourable.

The overactivity of the reticular system which has been suggested as the immediate cause of schizophrenia is, of course, a final common path, and there are many possible morbid processes which could give rise to such overactivity. The following are obvious possibilities:

1. A cortical focus could produce reticular overactivity by stimulating cortico-fugal pathways to the reticular system. The electroencephalographic evidence of focal activity might then be masked by the effect of the reticular system on the cerebral cortex. This would be the "forced normalization" of Landolt, and could account for the schizophrenic clinical pictures seen in epilepsy, brain injury and organic hallucinosis.
2. An irritative lesion of the reticular due to physical trauma, toxins or infection could produce overactivity of the reticular system. This would account for the schizophrenic clinical pictures seen in post-encephalitic states and possibly some post-traumatic schizophrenias.
3. An inherited biochemical disorder of the reticular system, such as a lack of mono-amino oxidase or some other enzyme could cause reticular overactivity. Like other enzymatic disorders, e.g. adrenal virilism, this need not manifest itself until puberty and might come to light because of physiological overactivity caused by an adolescent crisis.
4. An excess of adrenaline, nor-adrenaline, or similar substances produced by adrenal disorder could affect the reticular system. Such adrenal dysfunction

could be due to primary disease of the adrenal or to a hypothalamic or pituitary disorder, which in turn might be made worse by the adrenal disorder. Thus schizophrenia could be due to a neuroendocrine vicious circle.

Many other combinations of these disorders and other disorders could be postulated as causing the overactivity of the reticular system.

SUMMARY

It is suggested that the trema and early apophanous stage of acute schizophrenia can be explained as due to overactivity of the reticular system producing an undue diversion and disruption of the central process by the sensory input. Internal apophany and catatonic symptoms are explained as due to disruption of the central process with the formation of short-lived parallel central processes. Chronic schizophrenic process clinical pictures are considered to be due to a permanent parallel process which is dependent on a cortical reticular reverberatory system, while defect states without active symptoms are regarded as due to a reorganization of cell assemblies produced by the acute process. Pharmacological and electroencephalographic evidence in support of this theory is discussed. Possible causes of overactivity of the reticular system are briefly outlined.

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PERCEPTUAL ISOLATION AND SCHIZOPHRENIA

By

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H. THAKURDAS

and

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THE early investigators in the field of perceptual isolation were at McGill University, especially Bexton *et al.* (1954). Since then there have been many subsequent works and papers on the subject. In general, published results of this type of experiment can be divided into two groups. Sometimes marked sequelae to sensory deprivation have been described (Bexton *et al.*, 1954 and Goldberger *et al.*, 1958). Such disturbances included thinking, imagery and time appreciation, together with delusional and hallucinatory phenomena. On the other hand, at Princeton University, Vernon *et al.* (1956), under very carefully controlled experimental conditions, found virtually no sequelae. A paper published from this hospital (Smith and Lewty, 1959) described a silent room which was designed, constructed and standardized to a mean sound pressure level difference of 80 decibels. (Measurements made at $\frac{1}{3}$ octave intervals from 80–10,000 c./sec.) In this particular room 20 volunteers spent varying periods from 5 hours 50 minutes to 92 hours 20 minutes under conditions of partial and complete sensory deprivation. We used a room so as to mitigate the physical effects of reduced somatic activity and fur gloves were used instead of the more common cardboard cylinders, which after a period of time produce a great deal of skin irritation and indeed abrasions, and introduce a great deal of unnecessary complications into a study of pure sensory deprivation.

It was found in our 20 volunteers that apart from many individual reactions, a general sequence of events seemed to be followed: increased sleep—then restlessness and agitation—thinking difficulties—finally succeeded by panic. No post-isolation phenomena were seen. In this volunteer group were 11 women and 9 men; the average total deprivation of men was 29·24 hours and of women 48·70 hours.

Subsequent to our work done on the 20 volunteers, we decided to follow Harris's work (1959), on sensory deprivation associated with schizophrenia. Harris took 12 subjects and put them in a sound-proof box belonging to the B.B.C., a box which is well known and often used for various games played on the television. His actual period of isolation of these patients varied. In one case it was up to half an hour; in another up to 2 hours; in another up to 3 hours and another had a total period up to 8½ hours. These remarkably short periods of time produced three results:

1. That schizophrenics tolerated it remarkably well;
2. That all under-estimated time;
3. That their hallucinations were less vivid and troublesome during the time in the cubicle.

He concluded that in schizophrenics normal sensory stimuli reinforced hallucinatory experience in contrast to the effect of visual deprivation in mentally healthy individuals, where visual hallucinations can actually be produced.

We decided that as a result of this work it would be interesting to try the effect of perceptual isolation using our own standardized and measured methods on a group of schizophrenics. We defined schizophrenics as patients with illnesses of at least 5 years duration; they all must have had gross thought disorder, showing, for example, asyndesis and metonymy, and must also show impaired affect of one sort or another, as well as delusional and hallucinatory activity. In a word, they were typical long-term nuclear or process schizophrenics. We then felt that we would examine them according to the average total deprivation that we used for normal people, i.e., 29·24 hours in the case of men, and 48·70 hours in the case of women. Since results at the end of this time were non-existent or negligible, it was decided to continue much longer than originally planned. We examined their responses both during and after isolation to the 12 aspects of mentation which we had studied during the time that the normal volunteers were in. We also had varying psychological tests done before and after the period of isolation, and EEGs, also before and after. They were rated for 10 days previously on a Malamud-Sands Scale.

RESULTS

The following results were obtained in 6 schizophrenics:

(A) CLINICAL EXAMINATION

1. *Thinking*:

Our findings in normal volunteers showed that this generally became disordered—inability to concentrate to complete disorganization. In 12 it was this that produced panic—"I thought I was going mad".

In the schizophrenics: *No change*. Asyndesis and metonymy remained constant. Occasional further dilapidation noted but never persistent.

2. *Affect*:

One volunteer had a definite depression attack with crying. Others eventually became anxious and panicked.

In the schizophrenics: *No change*. Incongruous or flattened affect remained constant.

3. *Psychomotor Activity*:

Eighteen volunteers became agitated and restless and this seemed to be the forerunner of panic in nine of them.

In the schizophrenics: *No change*.

4. *Time*:

In normals usually *over-estimated* by between $\frac{1}{2}$ to 3 hours; except in 7 who initially underestimated. *All over-estimated at end*.

In Harris's group—all *under-estimated* time.

In our 6 schizophrenics—in beginning all *under-estimated*, then a few over-estimated. Then, when rhythm of meals became established, *all* were correct: *N.B.* If time of meal was deliberately varied at this stage, patients *over-estimated* (like normals).

5. *Body Image Disturbance:*

Seven volunteers experienced this, particularly arms and head: "My head is like a spinning cone going away from my body."

In schizophrenics: *No change.*

6. *Somatic Signs and Symptoms:*

Great efforts made to eliminate these by construction of room, etc., and gauntlets. Only 3 volunteers complained; the complaints were of 'socially approved' somatic manifestations of tension-anxiety.

In schizophrenics: *Nothing.*

7. *Irritability:*

Only 2 volunteers expressed irritability—one a heavy smoker; another an aggressive man.

In schizophrenics: *No evidence.*

8. *Sleep:*

Except for 2 volunteers who left the room early, after anxiety-tension within 6 hours, all slept more at beginning. Perfect illustration of Kleitman's theory that "sleep is induced by a decrease in afferent impulses from sensorium, producing a reversible inactivity of highest cortical centres".

In schizophrenics: *No real change in sleep pattern.*

9. *Dreams:*

In 5 normals nightmare quality, especially during anxiety period.

No schizophrenic admitted to any dreaming.

10. *Appetite:*

In volunteers appetite good in early stages. Then progressively diminished until, towards end, meals being left untouched.

In schizophrenics: *Invariably good.*

11. *Delusions:*

In volunteers nil, except fleeting paranoid ideas.

No change in number, type or intensity of those of the schizophrenics.

12. *Hallucinations:*

Probable illusions only in one volunteer.

In schizophrenics: *No change in number, type or intensity of established hallucinatory phenomena.*

13. *Time in Room:*

No.	Sex	Age	Time in Room (Hours)		
			P.S.D.*	C.S.D.	Total
1	..	M	46	18·51	467·43
2	..	F	40	20·35	466·50
3	..	M	31	22·41	409·10
4	..	M	30	20·52	464·33
5	..	M	28	19·60	420·10
6	..	F	40	20·35	466·50
					486·40

* P.S.D.=Partial sensory deprivation (when eating, at toilet, etc.)

C.S.D.=Complete sensory deprivation (complete isolation in room)

Total=A combination of above.

(B) PSYCHOLOGICAL EXAMINATION

The difficulty here has been to choose specific tests which might reveal the most significant effects, anomalies or abnormalities in view not so much of lack of theory as of the many theoretical issues involved at several levels.

For example, a simple psychophysical analysis of the *experimental* conditions makes us recognize also that the normal person, under normal conditions, can discriminate only over limited ranges and kinds of energy change in the physical world—in other words, that our *normal* condition is one of partial psychophysical deprivation. At a neurological level the central nervous system, if undisturbed by incoming signals, has been considered as in an ideal state of homeostasis, able to abstract maximum significance from the first “ripples” which pass through it (Grey Walter, 1953).

Perceptually each of the eight great common generalizations (Allport, 1955), obtained from an examination of thirteen major theories of perception, bears upon our experimental situation. Psychological theories of total personality which explain both schizophrenia and normal behaviour under the same set of principles are speculative and controversial.

Fundamental problems over management of the test interviews result from the reduced social stimulation and interpersonal interaction of the experimental experience. These required a test programme which maintained the maximum degree of partial isolation for as long as possible.

Since we have had only a few patients so far, and were not able to have the advantages of a psychologist during our initial volunteer phase, the test results so far can be considered tentative and exploratory.

1. *An Improvement in Response to some Cognitive Tests*

These tests were:

- (a) Mental Control (Wechsler Memory Scale).
- (b) Visual Reproduction (Wechsler Memory Scale)
- (c) Associate Learning (Wechsler Memory Scale).
- (d) Time Appreciation Test (Buck).
- (e) Mill Hill Synonyms.
- (f) Mitchell Vocabulary Scale.
- (g) Progressive Matrices.
- (h) Bender Visual Gestalt Test.

2. *An Alleviation of the Severity of some Signs of their Disorder*

- (a) More alert and involved in what was going on.
- (b) Less stereotyped, less rigid, less disciplined in response.
- (c) M.M.P.I. showed some improvement in validity.

3. *Jung's Word Association Reaction Times*

Slight but definite increase after isolation.

- 4. It is to be noted that these improvements and changes were not maintained 3 weeks later when tested again.

(C) EEG EXAMINATION

Four out of six pre-deprivation records were within normal limits. Two showed low medium voltage theta at 4-6 c./sec. and in one case paroxysmal delta at 2 c./sec. in both temporal areas.

Without exception, all records showed widespread slowing after deprivation. Slower frequencies involved alpha and also theta and delta. No paroxysmal activity noted. All records returned to their normal pattern subsequently.

CONCLUSIONS

These few results do agree with the thesis that schizophrenics can tolerate this type of anxiety provoking stress very well. All patients gave the impression of accepting these lengthy periods of isolation without any difficulties or upset. Such results—meagre though they are, raise a lot of questions and assertions.

Are we simply enhancing one of the natural defences used by schizophrenics, i.e. withdrawal in face of anxiety or anxiety-provoking situations, or is it simply another example of schizophrenic indifference in a situation which causes anxiety in normal people?

From a clinical point of view we were unable to confirm the results of Azima *et al.* (1956) who, in mentally-disturbed individuals, including schizophrenics, after 120 to 144 hours in a "single hospital room containing all facilities and situated farthest from the centre of the ward", found an increase in motivation, socialization and self-assertiveness, although it is to be noted that such changes were in fact found to a limited and non-persistent extent on the psychological tests. If anything, we found the opposite—i.e. almost catatonic stupor and certainly no increase in socialization or motivation. We therefore cannot really confirm his disorganization-reorganization hypothesis based on the use of perceptual isolation in the mentally ill. Neither did we find any evidence of a reduction of hallucinatory activity as observed by Harris (1959).

Normal subjects find great difficulty in adapting to or tolerating the stress of sensory isolation but schizophrenics find no difficulty. It appears to be their natural habitat.

Normal subjects find increasing need for and attempt to obtain extrinsic physical and social stimuli. Schizophrenics have no such need and make no effort to obtain such stimuli.

Such results add little to our knowledge of the causal nature of schizophrenia. Our results in normals likewise do not support the idea that the method produces an experimental model of the schizophrenic syndrome superior to the "model psychosis" of mescaline or LSD, as urged by Rosenzweig (1959).

The unresponsiveness of our schizophrenics (so far admittedly few in number) does suggest that withdrawal as a defence is possible in the face of such stress, but naturally would be untenable in any situation demanding task orientation or reality testing.

SUMMARY

1. Following upon experiences with normal volunteers in a specially constructed silent room, we tried the effect of such isolation on six chronic or nuclear type schizophrenics.

2. Very few positive results were achieved and it is likely that such perceptual isolation has little or no effect on the withdrawal pattern so characteristic of schizophrenics under stress or anxiety.

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DIFFERENTIAL EFFECTS OF COLOUR ON CHRONIC SCHIZOPHRENICS*

By

J. G. THORPE

FOUR experiments were carried out on chronic female schizophrenic patients in order to determine the differential effects of colour upon their behaviour. The three primary colours, red, blue and green, were used. The patients, who fell into Arieti's second or third stages of schizophrenia, had a mean age of 45 years (range 38-50), and a mean duration of hospitalization of 19 years (range 9-30). All experiments were carried out in a small side room painted white, the colour of which could be changed by the use of coloured lights hanging centrally from the ceiling. The colours used had a fixed intensity of 2 foot candles at a distance of 2 feet.

In the first experiment the following measures were obtained: word association reaction time, word association—bizarre responses, motor reaction time to a visual stimulus, and time estimation for periods of three minutes each.

In the second experiment psychogalvanic responses were obtained as follows: initial resistance of the palm, change in palmar resistance during a ten-minute period, and fluctuation of palmar resistance during the ten-minute period. All measurements were obtained from apparatus in an adjoining room.

In the third experiment each patient was asked to go into the room and to wait. The length of time she spent in the room was noted, patients being removed if they had not come out voluntarily within two hours.

The fourth experiment was a replica of the third except that the patients were ushered into the room by the ward sister rather than by the experimenter.

Each experiment was based upon a Latin Square design, patients experiencing each colour with colour sequences controlled. Analysis of Variance was used to evaluate the results.

The results were as follows:

Experiment I

There was no evidence to suggest differential colour effects upon the tests used.

Experiment II

No differential colour effects were detected.

Experiment III

The patients showed a differential colour response which was almost significant at the 5 per cent. level. They also showed a differential occasion response which approached significance at the 5 per cent. level. Patients remained longer in green (80 minutes) than in blue (60 minutes) and longer in

* This is a shortened version of a more detailed paper which can be obtained on request from the author.

blue than in red (52 minutes). In terms of testing occasions they remained in the room for 77 minutes on the first, 69 minutes on the second, and 51 minutes on the third, all occasions being irrespective of colour.

Experiment IV

Both the differential colour response, and the differential occasion response suggested in Experiment III disappeared, although the order of colours remained the same. The disappearance of these effects can be attributed either to the influence of the ward sisters who were in charge of the patients during the repeat test or to the influence of repetition *per se*. In both cases the conclusion must be that the colours used have not been shown to have any importance for the behaviour of this type of patient.

In conclusion, the present work suggests that none of the three primary colours has any appreciable effect on chronic schizophrenic patients in terms of the tests used. A number of problems remain. The first concerns the problem of assessing the differential effects of the whole range of the colour spectrum rather than of the primaries alone. Secondly the intensity of the colours used may be important. Thirdly, as patients are sometimes placed in wards on the basis of diagnosis, the relation between diagnosis and colour effects is important. Lastly, there is always the question of the nursing staff. Whether to organize the colours of wards in accordance with the effects of colour upon the nurses is a problem worthy of further research, as the general mood of the nursing staff must be reflected in their approach to the patients under their care.

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SOCIAL TREATMENT OF CHRONIC SCHIZOPHRENIA: A COMPARATIVE SURVEY OF THREE MENTAL HOSPITALS

By

J. K. WING

and

G. W. BROWN,

ONE of the fundamental concerns of social psychiatrists is to determine whether social events can influence the onset and the course of a particular mental illness and, if so, in what ways. The problems involved are formidable. In schizophrenia, for example, it is necessary to have reliable means of measuring the manifestations and progress of the disease, as well as adequate methods for assessing social events. However, if these difficulties can be satisfactorily overcome, there is a way in which a preliminary experiment can be made.

During the past 10 years, many mental hospitals in the United Kingdom have been changing rapidly, but in some the process of advance has been taken considerably further than in others. If it is true that social routines and administrative procedures influence schizophrenic symptoms, it should be possible to demonstrate differences in clinical state in patients from hospitals which have widely varied social conditions. Consistent associations between measurements of clinical and social factors in three mental hospitals chosen because they were likely to have made different degrees of progress towards the goal of the "therapeutic community", would at least provide a justification for more detailed investigation. If marked differences in social treatment were found at the three hospitals, but the clinical condition of patients did not vary, the theory of social influence would appear much less likely. Clearly, the hospitals would have to be comparable in certain important respects—in particular, that the schizophrenic patients referred to them should be equivalent in severity of illness, and that they should accept all such patients.

The present paper presents some results from a comparative survey of three mental hospitals which was made with the object of investigating these matters.

THE THREE MENTAL HOSPITALS

Three mental hospitals were selected for study because they seemed to differ markedly in social conditions and administrative policies, and their staff were likely to co-operate actively and closely.

Hospital A was a former county mental hospital which for many years had enjoyed a high reputation for its treatment of long-stay patients. At the end of 1951 there were 2,010 in-patients, and at the end of 1959, 1,860. The physician-superintendent himself supervised the two villas which housed the most severely ill chronic schizophrenic patients. A consultant psychiatrist supervised the active rehabilitation programme throughout the hospital.

Relatively less emphasis was placed on the admission hospital and on community services, although these were not backward. The hospital had a good all-round reputation but was particularly well known for its treatment of long-stay patients.

Hospital B was a former county borough mental hospital which was also well known for the quality of its in-patient treatment, but in contrast to Hospital A, the physician-superintendent devoted most of his time to the early treatment units and the community services. The long-stay wards were looked after by part-time general practitioners under the supervision of a consultant psychiatrist. At the end of 1951 there were 1,240 in-patients, and at the end of 1959, 940. The Superintendent specifically stated that he devoted most of his resources to community care and that his aim was to prevent patients from becoming long-stay by appropriate early treatment. Nevertheless, there were no locks on the doors of *any* of his wards and the hospital was internationally famous, like Hospital A, for its treatment of long-stay patients.

Hospital C was a former county mental hospital which had been passing through the period of change which came over many English mental hospitals during the post-war years. The process had been taken further in the male than in the female wards of the hospital. Throughout the institution, however, the features of the bad old mental hospital—padded rooms, side rooms used for seclusion, E.C.T., and sedation used instead of occupational therapy and skilled supervision to control disturbed behaviour—were disappearing, though a great deal remained to be done. A new physician-superintendent had begun work six weeks before the survey was made and had already introduced a number of changes. At the end of 1951 there were 1,720 in-patients, and at the end of 1959, 1,590.

SELECTION OF PATIENTS

The matron of each hospital was asked to supply a list of the names of all the female patients in the hospital together with age, length of stay, diagnosis and ward. A random sample was taken from each ward in Hospitals A and C of 120 female schizophrenics who had been resident more than two years and were aged under 60. Twenty of these acted as "spares". All the 73 women in Hospital B with these characteristics were selected. The diagnosis was checked from the case-notes and at interview with each patient. Four patients at Hospital A, none at Hospital B and one at Hospital C had to be replaced on diagnostic grounds. In addition a sample of case-notes at each hospital was checked to make sure that long-stay patients with other diagnoses were not in fact schizophrenic by the criteria used in this study—very few extra cases were discovered in this way and no change of procedure was necessary.

MEASUREMENT OF CLINICAL AND SOCIAL VARIABLES

(a) *Classification of Mental State*: A five-part classification—based on ratings of flatness of affect, poverty of speech, incoherence of speech, and coherently expressed delusions, made during a standard interview—has been described in another article (Wing, 1961). The subgroups formed were:

I. Moderately ill:

- 1a No symptoms at interview.
- 1b Moderate disturbance on any symptom, but no severe disturbance.
- 1c Severe flatness of affect, but only moderate disturbance on other symptoms (a borderline category).

II. Severely ill with florid verbal symptoms:

- 2 Coherently expressed delusions predominant.
- 3 Severe incoherence of speech predominant.

III. Severely ill without florid verbal symptoms.

- 4 Severe poverty of speech predominant.
- 5 Mute or almost mute.

The main criterion for distinguishing between moderately ill and severely ill patients was whether verbal symptoms intruded into replies to standard neutral questions (i.e. not specifically concerned with the patients' illness) to such an extent that conversation was markedly impaired.

The subgroups are defined so that they are "mutually exclusive and jointly exhaustive". The reliability of the system has been ascertained and is satisfactory for the present purpose. Patients in the various subgroups show different ward behaviour, and significantly different mean outputs on a simple industrial task.

Patients in subgroup 1a, who show no symptoms during the standard interview, may, of course, show symptoms at other times. However, 100 chronic schizophrenic in-patients in a good mental hospital did not show much change in symptomatology, according to the scales used, over a period of 6 months.

(b) *Behaviour on Ward*. A simple 12-item behaviour rating scale was also developed which yields two scores:

1. Social Withdrawal score (S.W.), composed of ratings on 8 items—slowness, underactivity, disinterest, social withdrawal, lack of conversation, poor mealtime behaviour, poor personal appearance, and incontinence. (Range of scores, 0–16.)
2. Socially Embarrassing Behaviour score (S.E.), composed of ratings on 4 items—overactivity, laughing and talking to self, threats of violence, and mannerisms. (Range of scores, 0–8.)

The ratings were made by senior ward nurses on the basis of observation of a sample week of the patients' behaviour in the ward. The S.W. score is very reliable between nurses and over periods up to 3 months; the S.E. score is reliable as between most—but not all—pairs of nurses, and shows considerable variability over periods longer than a week or so. The items composing the S.W. score all suggest a decrease in motivation to behave in a socially acceptable way, while the S.E. items suggest a positive drive to act in a socially embarrassing manner. There is a small correlation between the two scores ($r=0.19-0.30$ in various samples).

(c) *Attitude to discharge* was rated on a 5-point scale on the basis of answers to standard questions:

Definitely wants to leave. Vaguely wants to leave. Ambivalent. Indifferent. Definitely wants to stay.

(d) *Ward Restrictiveness Scores*. The female wards in which more than one-fifth of the patients were schizophrenic, aged under 60, and had been resident more than two years, were selected for special study. Ward sisters (and often patients and other nurses) were asked about limitations on the movements of patients and about the extent of their everyday responsibilities. Forty brief scales representing different aspects of life in the ward were constructed. A total of 61 points indicated a maximum restrictiveness score—the minimum was 0 points. The following are examples of the kind of information rated:

- (i) Time at which the ward was locked at night.
- (ii) Whether patients could go to hospital entertainments without permission of the staff.
- (iii) Whether patients were kept waiting at the meal table until all had finished (e.g. for grace).
- (iv) Whether any were allowed to use the ward kitchen for minor tasks such as tea-making.
- (v) Whether they had ready access to stored private clothing.

(e) *Time Budget.* The sister in charge of the ward was asked, in considerable detail, about the time spent by each patient in the sample in various activities on the last weekday before the interview. The aim was to cover the whole day to within a quarter of an hour. If necessary to supplement this information, the patient was also interviewed.

(f) *Personal Possessions of the Patient.* An inventory of the personal possessions of each patient (whether initially provided by the hospital, or privately) was obtained by interviewing the patient and the ward sister. Strict ownership was not required; continuous and sole use was considered to define personal possession. For example, if hospital underclothing was returned to the same patient each time after laundering, it was classed as personal property.

(g) *Nurses' Opinions about Patients.* Ward sisters and other nurses were asked standard questions about each of the patients in the sample in order to obtain two opinions about each patient's ability to cope with certain everyday activities and responsibilities.

(h) *Other Information.* A great deal of other information was gathered about hospital policies by a number of standardized and unstandardized techniques. This information will not be systematically presented here.

RESULTS

1. AGE, LENGTH OF STAY, AND OCCUPATION OF FATHERS

The composition of the three samples in respect of age and length of stay is set out in Tables I and II.

TABLE I
Hospitals by Age of Patients

Age (Years)								A	B	C
20-40	..	..	..	..	..	..	..	30	18	26
41-50	..	..	..	..	..	..	..	35	29	30
51-60	..	..	..	..	..	..	..	35	53	44
Total	..	..	..	..	..	..	..	100	100*	100

* Per cent. of 73.

TABLE II
Hospitals by Length of Stay of Patients

Length of Stay (Years)								A	B	C
2-10	..	..	..	..	..	..	..	42	29	30
11-20	..	..	..	..	..	..	..	38	33	40
21+	..	..	..	..	..	..	..	20	38	30
Total	..	..	..	..	..	..	..	100	100*	100

* Per cent. of 73.

The distributions are not significantly different at the three hospitals (for age, $\chi^2=6.40$, $df=4$, $p=>.10$; for length of stay, $\chi^2=8.62$, $df=4$, $p=>.05$). However, since Hospital B tends to have more patients in the 51-60 age group, and more in the over 21 years length-of-stay group, it will be necessary to allow for this in the subsequent analysis.

The occupation of the patients' fathers was classified according to the system of Hall and Jones (1950), and differences between the three hospitals are shown in Table III.

TABLE III
Hospitals by Occupation of Patients' Fathers

	A	B	C
Professional and Managerial	36	14	21
Lower Administrative and Clerical	8	8	16
Skilled Manual	21	16	16
Semi-skilled Manual	6	21	10
Unskilled Manual	10	18	22
Not known	19	23	15
Total	100	100*	100

* Per cent. of 73.

An effort was made to obtain this information by writing to the relatives of patients who could not give it themselves, but it was not available for 51 patients (19 per cent.). There was considerable variation between hospitals. ($\chi^2=29.54$, $df=8$, $p=<.001$). In particular, the fathers of patients at Hospital A had more often been engaged in professional or managerial occupations compared with the other two hospitals.

2. MENTAL STATE, WARD BEHAVIOUR AND ATTITUDE TO DISCHARGE

(a) *Classification of Mental State*

Table IV shows that there were significant differences in the distribution of the clinical subgroups at the three hospitals ($\chi^2=25.81$, $df=8$, $p=<.01$). There were fewer moderately ill patients in the sample from Hospital C (23 per cent. compared with 39 per cent. in Hospital B and 40 per cent. in Hospital A). On the other hand, there were 56 patients at Hospital C who showed severe flatness of affect or poverty of speech—subgroups 4 and 5—compared with 39 per cent. at Hospital B and 26 per cent. at Hospital A.

TABLE IV
Hospitals by Clinical Classification of Patients

Clinical Category	Hospital A	Hospital B	Hospital C
<i>Moderately ill:</i>			
1a No symptoms	10	15	8
1b Moderate symptoms	21	19	13
1c Borderline severe symptoms	9	5	2
	40	39	23
<i>Severely ill:</i>			
2. Coherent delusions	17	8	6
3. Incoherence of speech	17	14	15
4. Poverty of speech	20	25	32
5. Mute	6	14	24
	26	39	56
Total	100	100*	100

* Per cent. of 73.

There was a significant association between clinical classification and length of stay in hospital ($\chi^2=21.71$, $df=8$, $p=<.01$). In particular, relatively few patients in subgroups 1 and 2 had been in hospital 21 years or more, and relatively few patients in subgroup 5 had been in hospital for less than 10 years. Since there were fewer patients at Hospital A who had been in hospital 21 years or more, or who were aged 51–60, than at the other two hospitals, the age and length-of-stay distributions at Hospitals B and C were standardized to those of Hospital A. This reduced the numbers of patients at Hospital B in subgroups 4 and 5 to 32 per cent.—very little different to the proportion at Hospital A. At Hospital C, however, the deficiency of patients in subgroup 1 (26 per cent. when corrected) and the excess in subgroups 4 and 5 (51 per cent. when corrected) still remained.

Table V shows that there is no significant association between clinical classification and occupation of patients' fathers ($\chi^2=11.92$, $df=12$, $p=>.30$). This held true for each of the hospitals taken separately, and also when the non-manual occupations were broken down further into professional, managerial, administrative, supervisory and clerical.

TABLE V
Clinical Classification by Occupation of Patient's Father
(Data for 3 hospitals combined)

				Non-manual	Skilled Manual	Semi-skilled Manual	Unskilled Manual
1	..	..	..	35	24	9	14
2	..	..	..	13	5	3	3
3	..	..	..	11	7	6	4
4	..	..	..	25	11	9	18
5	..	..	..	13	2	4	6
				97	49	31	45

(b) *Behaviour in Ward*

The mean S.W. scores for the samples of patients from Hospitals A, B and C were 2.9, 4.6 and 5.4, respectively ($F=10.0$, $p=<.001$). Thus patients at Hospital A showed least abnormality in this respect. The mean S.E. scores were 1.2, 2.3 and 1.3, respectively ($F=9.8$, $p=<.001$): that is, patients at Hospital B showed most disturbance on this scale.

A special check was necessary because 34 per cent. of patients in Hospital B, and 21 per cent. of patients in Hospital C, came from one large long-stay ward. If the two ward sisters concerned had been unduly severe in their ratings of ward behaviour, they could have biased the comparison with Hospital A. Their ratings were therefore compared with ratings made by other ward sisters in the same hospital, and with the mental state ratings made independently by the investigator. There was no indication that a special bias was operating in either case.

Table VI shows mean S.W. and S.E. scores in three length-of-stay groups. Analysis of variance of S.W. scores disclosed significant differences between hospitals ($F=10.8$, $p=<.001$) and between length of-stay-groups ($F=11.7$, $p=<.001$). Subsequent t-tests showed that there was a significant increase in score with length of stay. Patients in Hospital C showed significantly higher mean scores than patients in Hospital A, however long they had been in hospital.

However, there was a gradation in mean score at Hospital B. Patients who had been resident 2-10 years were not significantly different from equivalent patients in Hospital A. Patients who had been resident more than 21 years were not significantly different from equivalent patients in Hospital C. The remaining patients, who had been in hospital 11-20 years, had a mean score which was intermediate between those of equivalent patients in Hospitals A and C.

There was no significant variation in S.E. score with length of stay in hospital.

TABLE VI

Mean S.W. and S.E. Scores by Hospitals and by Length of Stay of Patients

Length of Stay in Hospital	Mean S.W. Score				Mean S.E. Score			
	A	B	C	Total	A	B	C	Total
2-10 years ..	2.2	2.6	3.9	2.9	1.0	1.7	1.5	1.3
11-20 years ..	2.8	4.5	6.1	4.5	1.3	2.3	1.3	1.5
21+ years ..	4.6	6.3	5.9	5.7	1.4	2.6	1.2	1.8
Total ..	2.9	4.6	5.4	4.3	1.2	2.3	1.3	1.5

Mean S.W. and S.E. scores for patients whose fathers were in various occupational groups, are shown in Table VII. Combining equivalent groups from the three hospitals, there was significant variation in both scores (for S.W. scores, $F=3.26$, $p<.05$; for S.E. scores, $F=4.32$, $p<.01$). Subsequent t-tests showed that this was mainly due to the fact that patients whose fathers were skilled manual workers tended to have lower scores than other groups (this was also true of the three hospitals taken separately). There was no tendency for the prestige (according to the Hall Jones scale) of the father's occupation to be associated with the degree of disturbance of the patient's behaviour.

TABLE VII

Mean S.W. and S.E. Scores by Hospitals and Occupation of Patients' Fathers

Hall-Jones Occupational Category		Mean S.W. Score				Mean S.E. Score			
		A	B	C	Total	A	B	C	Total
1, 2, 3	Professional, managerial ..	3.1	3.0	5.3	3.7	1.1	1.3	1.1	1.1
4, 5	Lower supervisory and clerical	4.1	3.8	5.3	4.7	3.0	2.0	1.8	2.1
6	Skilled manual ..	1.7	2.7	3.4	2.5	0.8	1.7	0.8	1.1
7	Semi-skilled manual ..	3.5	5.1	6.5	5.3	1.3	2.7	2.2	2.3
8	Unskilled manual ..	2.6	4.0	5.5	4.5	0.4	2.1	1.4	1.4
-	Not known ..	3.4	7.5	6.9	5.7	1.4	3.3	1.1	1.7

(c) Attitude to Discharge

Fifty-four per cent. of patients in Hospital A, 66 per cent. in Hospital B and 70 per cent. in Hospital C were either indifferent about being discharged or actually wanted to stay ($\chi^2=5.81$, $p=\text{just over } 0.05$). There was an association between clinical classification and attitude to discharge. If patients who were indifferent were omitted, 62 per cent. of moderately ill patients and patients with coherently expressed delusions (subgroups 1 and 2) had some desire to leave, compared with 49 per cent. of the remaining groups. Indifference was, of course, strongly related to poverty of speech and muteness. The relationship

between attitude to discharge and length of stay is shown in Table VIII, which also shows the detailed results for the three hospitals. There was a decrease in the proportion of those wishing to stay, the longer patients had been resident, in each of the three hospitals (combined $\chi^2=16\cdot9$, $p=<0\cdot001$). When either clinical condition or length of stay was controlled, there was no difference between hospitals.

TABLE VIII
Attitudes to Discharge by Hospitals and Length of Stay

Attitude to Discharge			Length of Stay (Years)								
			2-10			11-20			21+		
			A	B	C	A	B	C	A	B	C
Some desire to leave	..	..	14	6	12	9	4	6	3	1	-
Ambivalent	..	..	11	4	3	7	6	4	2	4	5
Indifferent	..	..	7	6	9	10	4	23	8	14	13
Desire to stay	..	..	10	5	6	12	10	7	7	9	12
Total	..	..	42	21	30	38	24	40	20	28	30

3. SOCIAL AND ADMINISTRATIVE PROCEDURES

(a) *Ward Restrictiveness Scores*

Each patient in the three samples was given the score for his ward, and the sum of scores was divided by the number of patients in the sample. The mean scores are expressed as percentages of the possible total of 61. The average score for Hospital A was 27 (range 8-62, 8 wards studied). For Hospital B it was 40 (range 20-54, 5 wards studied), and for Hospital C, 66 (range 11-89, 8 wards studied). These mean scores represent the combined experience of the patients in the various samples.

Measures of amenities provided on these wards also showed large differences between the hospitals. For example, a count was made of all lockers (whatever their size), wardrobes, dressing tables and chests of drawers on each of the selected wards. In Hospital A there were 522 such articles of furniture for 378 patients in the 8 wards studied (1·4 per patient), in Hospital B 418 articles for 191 patients in 5 wards studied (2·9 each), and in Hospital C 113 articles for 500 patients in 8 wards (0·2 each).

(b) *Time Budget*

Table IX gives details of the average time spent, by patients in the three samples, on certain daily tasks. Hospitals A and B are fairly similar. Hospital C differs from them very markedly; in particular, patients spent a great deal of time in the ward (only 30 per cent. left the ward at all) and the time spent either doing "nothing" (5 hours 36 minutes) or at "toilet" or "meals" (3 hours 10 minutes) when in the ward is very long compared with the other two hospitals. An average of 67 per cent. of the patients' total time out of bed was accounted for under these two headings. (The patient was only regarded as doing "nothing" when there was no evidence that she was occupied in any way which could possibly be called constructive or leisure activity.) A high score (e.g. nine hours) would be recorded for a patient who remained inert the whole day on the same chair she occupied at breakfast; a lower score (e.g. four hours) by one who did some ward work during the day but after tea sat in a chair until bedtime, neither talking, knitting, reading nor watching television, etc. In case of any doubt, judgment was given against a recording of "nothing".

TABLE IX
Time Budget of Typical Weekday

	A		B		C	
	hours	mins	hours	mins	hours	mins
On Ward:						
Leisure activities	2	04	1	03	1	22
Ward work or occupational therapy ..	1	19	1	17	1	45
Toilet or meals	2	38	2	48	3	10
Nothing	2	40	2	40	5	36
	8	41	7	48	11	53
Off Ward:						
Occupational therapy or leisure ..	0	41	1	16	0	20
Work	3	32	2	50	0	47
Nothing	0	08	0	35	0	03
	4	21	4	41	1	10
Total time out of bed	13	02	12	29	13	03

Hospitals A and B differed in two important ways. Firstly, at Hospital B considerably less time was spent in leisure activities (particularly watching television, though this was available on all wards). The explanation appears to be a much earlier bedtime—7.38 p.m. compared with 8.30 p.m. at A and 8.13 p.m. at C. Secondly, somewhat fewer patients in Sample B worked for at least three hours per day. "Work" was defined as occupation in the grounds, or in hospital departments, special industrial work or outside employment, and excluded ordinary occupational therapy.

(c) *Personal Possessions*

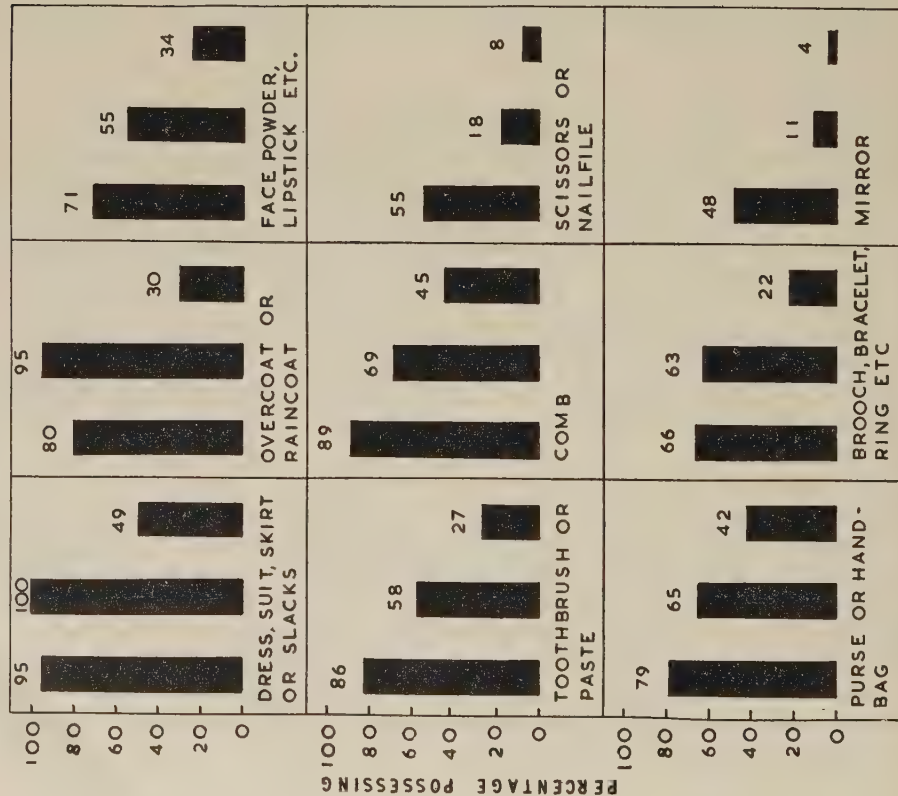
The main differences appeared between Hospital C and the other two. For example, 33 patients in Hospital C had *no* personal possessions at all (44 if items of clothing are not considered), whereas none at Hospital B and only one at Hospital A were without any effects at all.

Some detailed results are presented in Figure 1. While patients at Hospitals A and B possessed comparable amounts of clothing, Hospital B occupied a more intermediate position when other articles were considered. In figures for possession of a comb or brush, for instance, B (69 per cent.) occupied an intermediate position between A and C (89 and 45 per cent.), while the numbers owning scissors or a nail file at A (55 per cent.) far exceeded those at either B or C (18 and 8 per cent.).

(d) *Nurses' Opinions about Patients*

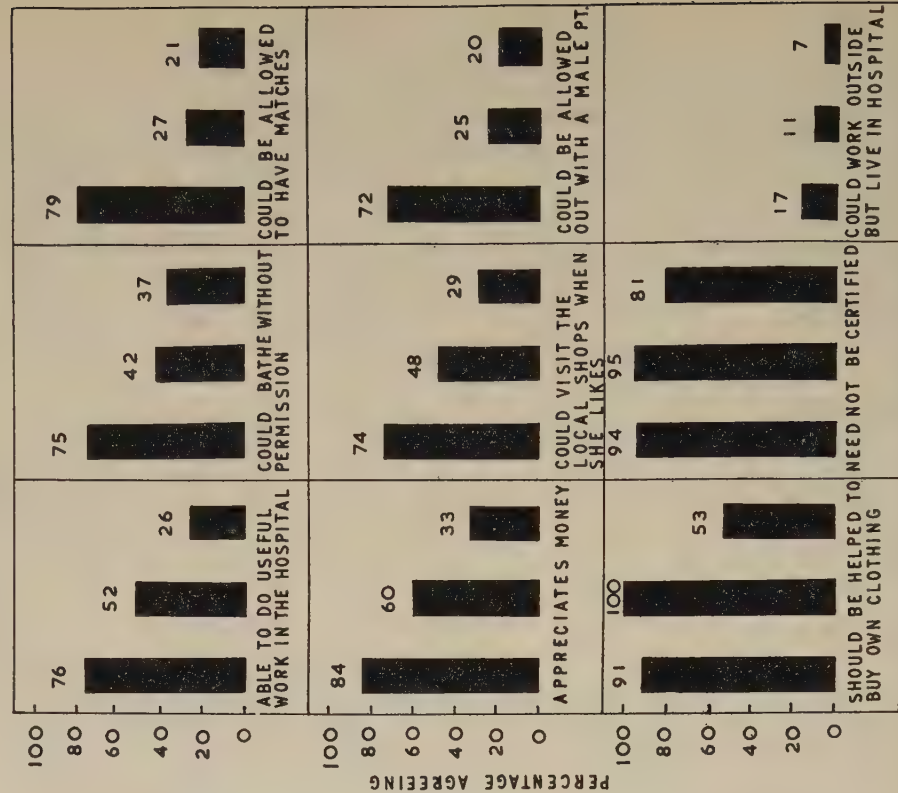
Ward sisters at Hospital A were more optimistic in their opinions about the patients than those at either B or C (see Fig. 2). Nurses at Hospital B tended to be intermediate or to approach those at Hospital C very closely. For example, 76 per cent. of patients in Sample A were thought capable of doing useful work (laundry, domestic work, etc.) for the hospital compared with 52 per cent. at Hospital B and only 26 per cent. at Hospital C. There was a high degree of agreement between nurses at all three hospitals on only two questions. Few patients were considered to need certification, and few were thought to be fit enough to work outside the hospital while living in.

FIGURE 1 - PERSONAL POSSESSIONS (SUPPLIED PRIVATELY OR BY THE HOSPITAL) OF LONGSTAY FEMALE SCHIZOPHRENIC PATIENTS



IN EACH CELL THE BLOCKS REPRESENT HOSPITALS A, B AND C IN THAT ORDER.

FIGURE 2 - WARD SISTERS OPINIONS ABOUT LONGSTAY FEMALE SCHIZOPHRENIC PATIENTS



IN EACH CELL THE BLOCKS REPRESENT HOSPITALS A, B AND C IN THAT ORDER.

There was a high level of agreement between the opinions of ward sisters and nurses when discussing the same patients.

(e) *Drug Treatment*

One other item of information which was systematically collected for all patients in the samples will be considered here. The ward sisters were asked to say what drug treatment the patients had been receiving during the previous week. The detailed results are presented in Table X.

TABLE X

Drugs Prescribed for Chronic Schizophrenic Patients in the Three Hospitals

		Hospital					
		A		B		C	
Number in sample		100		73		100	
Number receiving any day-time drug ..		63		62		70	
Drug Received	Size of "Unit"	Total Daily		Total Daily		Total Daily	
		N	"Units"	N	"Units"	N	"Units"
Chlorpromazine	100 mg.	23	44	33	76	44	94
Reserpine	1 mg.	13	47	18	57	—	—
Fentazine	4 mg.	—	—	7	22	4	14
Prochlorperazine	25 mg.	8	16	14	36	2	9
Trifluoperazine	10 mg.	24	42	1	1	7	8
Unnamed compound	50 mg.	—	—	—	—	10	20
Total "Units"		—	149	—	192	—	145
				(For 100 patients 262)			
Sodium amytal (by day)		5	20gr.	16	109gr.	8	60gr.
Phenobarbitone (by day)		3	5gr.	2	3gr.	3	9gr.
Sodium amytal (at night)		12	61gr.	6	24gr.	12	42gr.

More patients at Hospital B were receiving major tranquillizers or sedatives, by day, than at the other two hospitals. In order to provide a direct comparison, doses of the major tranquillizers were translated into arbitrary units, roughly equivalent to 100 mg. of chlorpromazine. A total of 149 units daily was prescribed for the 100 patients at Hospital A, 192 units for the 73 patients at Hospital B (i.e. 262 for 100 patients), and 145 units for the 100 patients at Hospital C. Sodium amytal was also prescribed in larger quantities, by day, at Hospital B than at the other two hospitals.

COMPARISON OF MATCHED PATIENTS

Twenty patients at each hospital were selected (without knowledge of their scores on these social measures) because they were equivalent in clinical grouping, S.W. score, S.E. score, age, length of stay in hospital and attitude towards discharge. The clinical condition of these patients was considerably above the average for the complete samples. Differences between hospitals, in terms of social measurements, were reduced when only these 20 patients were considered, but for the great majority of items tested large differences still remained between Hospital C and the other two. There was, for example,

little difference between the numbers leaving the ward for any reason in Hospitals A and B (85 and 80 per cent. respectively), but in Hospital C only 45 per cent. had done so. Differences between Hospitals A and B tended to be much reduced, but on some items they were still significant. These included certain attitudes of the nursing staff, for example, towards possession of matches, bathing alone, and going out with a male patient, in all of which staff at Hospital B were more doubtful.

SUMMARY OF DIFFERENCES BETWEEN HOSPITALS

There were differences between the hospitals in respect of the age, length of stay, and social class composition of the samples of long-stay schizophrenic patients studied. When these were allowed for, there was little difference in the clinical classification of the samples from Hospitals A and B, but Hospital C contained significantly fewer moderately ill patients. Twenty-four per cent. of the patients in Hospital C were mute at interview, but only 6 per cent. at Hospital A. Ward behaviour was significantly more characterized by social withdrawal in Hospitals B and C than in Hospital A. Patients in Hospital B, according to the S.W. scores, behaved like those from Hospital A before they had been in hospital ten years, and like those from Hospital C in the longer-stay groups. S.E. scores on the other hand were highest at Hospital B. Attitudes to discharge also varied significantly between hospitals, mainly due to a relationship with clinical condition and length of stay.

In general, the sample from Hospital A showed least disturbance in verbal behaviour at interview, and in ward behaviour, while the sample from Hospital C showed most. Patients in Hospital B were in some respects like the former, in some respects like the latter.

Social differences between hospitals were evident both in matters of central importance in everyday life, and in small details. For example, there was a 50 per cent. difference between Hospitals A and C in the proportion of the sample leaving the ward during the day and a 60 per cent. difference in the proportion of patients possessing a toothbrush. Hospital B, on the whole, approximated to Hospital A, but also showed certain similarities with Hospital C. In matters of obvious importance, such as the amount of time spent off the ward, the opportunities for constructive work, the provision of respectable personal clothing, the amount of locker space, etc., Hospital B was more or less equivalent to Hospital A. But in more intimate, and perhaps less obvious, details, the patients in Hospital B were not so well off as those in Hospital A. Fewer patients, for example, possessed scissors or a mirror, and fewer baths were "screened" from onlookers. Patients went to bed on average nearly one hour before those at Hospital A and leisure activities were more restricted. These differences were further reflected in the opinions of the nursing staff. Ward sisters at Hospital B thought that *all* patients should be helped to buy clothing (91 per cent. did at A) but that only 27 per cent. could be trusted with matches (79 per cent. at A).

DISCUSSION

The results given above show a consistent pattern in the three hospitals studied. Patients in the sample of female long-stay schizophrenics from Hospital A tend to show least disturbance in verbal behaviour at interview, and least disturbance of social behaviour in the ward (assessed by S.W. score), while patients in the sample from Hospital C tend to show most disturbance on these

two variables. Hospital B patients are intermediate in both respects, though when age and length of stay are allowed for, they approximate more closely to patients in Hospital A so far as verbal behaviour at interview is concerned. The difference in ward behaviour between Hospital A and Hospital B patients is confined to those who have been resident over ten years: in this very long-stay group, ward behaviour (S.W. score) is closer to that in Hospital C. S.E. scores are significantly higher at Hospital B than at the other two hospitals, particularly in the longer-stay groups. No interpretation of this finding can be offered at this stage of the analysis.

The same pattern that is evident in the measurements of mental state and socially withdrawn behaviour is also clearly recognizable in the social data. Hospitals A and C are markedly different in nearly every detail—ward restrictiveness, time budget, personal possessions and nurses' opinions—while Hospital B on some measurements shows similarity to Hospital A, and on others to Hospital C.

This report has presented only the barest minimum of information—all of the descriptive details which can give substance to this brief formulation have been omitted, as have many systematic observations. In addition, no attempt has been made, at this stage, to analyse the interactions between the clinical and social measurements. However, the major problem of interpretation can be stated immediately. Are the different social conditions under which the patients live responsible for the differences in their clinical condition in the three hospitals, or are there different processes of selection which ensure that more clinically disturbed patients accumulate in one hospital than in another? If the latter process is operating, the high proportion of severely-ill patients might determine the social and administrative policies of the hospital rather than the other way round. In fact, the extent to which treatment procedures had to be adapted to clinically disturbed patients might lead to generalization of these procedures to cover even moderately ill patients. This would account for the fact that differences between the hospitals in respect of personal possessions, ward routines and nurses' opinions persist even within clinical categories, and when age and length of stay have been allowed for.

It might be objected that a patient who was offered the choice of admission to a chronic ward in one of these hospitals would consider this a somewhat academic argument. There is little doubt on the evidence that, though she might hesitate between Hospital A and Hospital B, she would reject Hospital C at once. However, the point at issue is not which hospital provides most comfort for its chronic schizophrenic patients, or which a discriminating patient would most like to live in. The question is—Do the differences in social atmosphere indicate that one hospital will have a beneficial, while another has a harmful, effect on the mental health of its inmates (irrespective of whether they like it there or not)? The answer to this question should not be taken for granted because it seems obvious on humanitarian grounds.

The data collected in the present "cross-sectional" survey are not very well suited to answering questions of this kind. However, a number of checks may be made, and two kinds of "longitudinal" data can be gathered which may help to provide an answer. The two alternatives may be taken separately—Does the environment influence the patients, or do the patients create their own environment? In the latter case, it should be possible to demonstrate some kind of selective process at work, above all in Hospital C, to account for the high proportion of clinically disturbed patients there. Two possibilities suggest themselves. The social class structure of the three communities is very different

and possibly a different type of patient is being admitted to Hospital C. This argument cannot be carried very far since Hospital B admits from an area which is similar in social composition and, in any case, clinical classification and ward behaviour are not related to the occupation of the patients' fathers. The case notes of all schizophrenic patients entering Hospitals A and C have been studied and rated, and no evidence was found that the latter hospital admitted more severely ill patients. Another possibility is that there is a relatively high early discharge rate in Hospital C, so that the severely ill patients, with a bad prognosis, who remain and accumulate in the hospital, would form a high proportion of all patients who had been resident for more than two years. Alternatively, there might be a specially high number of discharges of moderately ill chronic patients from Hospital C, with the same result. The latter suggestion has been checked and it is found that Hospital A has had by far the highest annual discharge rate of chronic schizophrenic patients during the past 10 years. Studies have also been made of cohorts of admissions of female schizophrenic patients to the hospitals in 1951 and 1956. The statistical analysis is not yet complete but there appears to be no evidence that Hospital C has had a higher early discharge rate than the other two hospitals.

If anything, therefore, these results seem to point in the opposite direction. But even if there had been a specially high early discharge rate at Hospital C, which distinguished it from the other hospitals, it is not clear that this would necessarily mean an accumulation of a large proportion of severely ill patients. Hospital B had the lowest proportion of patients retained for two years, of the three hospitals, but 39 per cent. of the long-stay schizophrenics were moderately ill. Thus, not only severely ill patients are retained (and, probably, not only moderately ill patients are discharged).

There is a further method of checking the selection hypothesis. If the survey can be repeated in Hospitals B and C, after the lapse of a suitable period of time, using the same individuals, selective factors will be held constant. Any changes that have taken place in clinical condition would therefore be attributable either to the lapse of time alone or to the introduction of changes in social conditions or in physical treatment. It can very fairly be assumed that time alone, up to now, has not seen very marked changes in the clinical condition of these chronic patients (unless it has been for the worse). Most of them were already receiving adequate doses of the new tranquillizing drugs and no major development in this direction has taken place. A definite test of the general hypothesis is therefore possible, and the necessary further work is being undertaken.

Whatever the outcome of these studies, it is already quite clear that there are major social differences between mental hospitals, and that at least one specific group of in-patients also varies markedly in type and severity of illness. It is not necessary to wait for general agreement on a theory to account for the causes of these differences before appreciating their administrative significance.

SUMMARY

The clinical condition of samples of chronic female schizophrenic patients in three mental hospitals was assessed by means of standard interviews, and rating scales completed by ward sisters. Information on the restrictiveness of the ward routine, the way the patient's day was organized, her personal possessions, and the nurses' opinions of her, was also systematically gathered. After due allowance had been made for differences in distribution of age,

length of stay, and social class (occupation of father), marked differences still remained between the groups. A consistent pattern emerged. At Hospital A, where the main emphasis of care was on the long-stay patient, there was least clinical disturbance and most personal freedom, useful occupation, and optimism among the nursing staff. At Hospital C, where reform had not progressed so far, there was most clinical disturbance among patients and least personal freedom, useful occupation and optimism. Hospital B was intermediate in most respects. Alternative explanations are considered and it is concluded that there is good preliminary evidence that social conditions in a mental hospital do influence the mental state of schizophrenic patients. It will be necessary to show an improvement in patients in Hospital C, as the social regime there changes, in order to confirm these results.

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A SIMPLE AND RELIABLE SUBCLASSIFICATION OF CHRONIC SCHIZOPHRENIA

By

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No detailed and comprehensive system of subclassification of chronic schizophrenia has been generally adopted in this country. Most psychiatrists are content to use the broad categories of simple, hebephrenic, catatonic, paranoid and undifferentiated schizophrenia. Placing a patient into one of these subgroups entails a judgment which is based upon all the information at the clinician's disposal. Such data are necessarily unstandardized; the method of collection varies from doctor to doctor and from patient to patient, and the criteria of classification are not uniformly agreed upon. No objective tests are available which would serve to check the accuracy of the decisions taken. It is inevitable, in these circumstances, that clinical systems of subclassification should not be much used for scientific purposes. The basic essential of a classifying instrument is that it should produce the same results in the hands of different investigators, and that its standards of measurement should not vary from one occasion to another. No study so far published has attempted to deal with this problem, which becomes more serious as the elaboration (and possibly the clinical usefulness) of the classification increases. Leonhard's system (1, 4, 5) for example, would gain enormously if its reliability could be demonstrated.

A method of classifying chronic schizophrenic patients which could be simply, rapidly and consistently applied would be valuable for a number of purposes, notably for describing and comparing hospital populations and for delineating subgroups for psychological and sociological study. The present work was designed to show whether a very simple classification, based on leading symptoms assessed during a brief psychiatric interview, could be used consistently by different psychiatrists, and whether chronic schizophrenic patients would need to be reclassified after a period of six months. An attempt was also made to discover whether patients in the defined subgroups showed differences in their ward behaviour, as independently observed and rated by senior nurses. The process of increasing reliability inevitably entailed restricting and standardizing the information obtained. A great deal of material which a clinician would think relevant was excluded and, for this reason, the system was not intended for clinical use on its own.

DEVELOPMENT OF SYMPTOM RATING SCALES

Four typical attributes of schizophrenic mental state were chosen, each of which could be rated on the basis of information obtained during a clinical examination of the patient. These symptoms were flatness of affect, poverty of speech, incoherence of speech and coherently expressed delusions. An attempt was made to define each symptom and to construct a 5-point scale for rating it. No behavioural items were included since insufficient information could be obtained during a short interview. The scales are presented in Appendix A.

The ratings were made on the basis of a partially-standardized interview.

During the first ten minutes or so the patient was asked how he felt about life outside hospital, and what plans he had for returning to the community. A series of questions about life in the hospital, and about topics of general interest, followed. The same questions were asked of each patient and usually in the same order. Replies were recorded. This part of the interview provided a sample of speech about relatively neutral topics. Then certain questions referring to possible mental symptoms were always put. The patient was asked how he got on with other people, whether anyone seemed to be against him or tried to do him harm or talked about him, whether he could think clearly, whether there was any interference with his thoughts, whether he had experienced anything in the nature of hypnotism or telepathy or electricity playing on his mind, whether his thoughts were read, whether there was any reference to him on the radio or television, whether he had any special powers or abilities, whether he was specially religious, whether he heard voices or saw visions, whether events seemed strange or puzzling, and whether things looked and sounded normal. Supplementary questions were asked as necessary and such specific questions as seemed likely, from a study of the case notes, to bring out the appropriate psychopathology. The interview usually lasted from 20 to 30 minutes, depending on how much the patient was prepared to converse. It is not suggested that this constituted a full examination of mental state: only certain aspects were examined systematically. Ratings could be made on three of the four scales largely according to the recorded replies to the standard questions. Flatness of affect would mainly be rated by a subjective judgment on the part of the interviewer. The four symptoms are described in more detail below.

Flatness or Incongruity of Affect

This trait was the most difficult to define objectively. A positive rating meant that there was a gross incongruity between the content of speech and the concomitant affective reaction, or that there was a gross lack of normal emotional involvement in the interview, or that there was inappropriate giggling without warmth of affect. The various degrees of ratings are shown in Appendix A. Clearly certain patients might be rated under this heading who were grossly apathetic or indifferent for other reasons than schizophrenia. The main serious source of error was likely to have been the apathy that is said to accompany "institutionalism". If this can be severe enough to mimic schizophrenic flattening, it would be rated on the present scale. In addition, certain patients who had a moderate degree of flattening due to their illness might have been rated as showing severe flattening because of a similar effect of "institutionalism".

Poverty of Speech

A rating of (5) on this scale was only made when the patient was mute throughout the interview, or spoke only two or three words. The patient whose answers were monosyllabic, often with long pauses or a failure to answer at all, was rated (4). Other patients were rated (4) because, although there was a reasonable amount of speech, the answers were so slow and hesitant, so vague and lacking in content, so repetitious and wandering, that meaningful conversation was almost impossible. A rating of (2) or (3) was more difficult to make. The patient was given the benefit of any doubt, and deficiencies of education and lack of practice in social intercourse were assumed in all cases. That is to say, poverty of speech had to be very marked before a positive rating was made.

Extracts from conversations with a patient rated (4) are given below:

- | | |
|-------------------------------------|----------------------|
| Q. How long have you been here? | A. Six years. |
| How do you like it? | Quite nice. |
| Do you want to leave? | (No answer). |
| How about leaving hospital? | I did go on holiday. |
| Would you like to leave altogether? | I don't know yet. |
| Are you content to stay? | Yes. |
| Have you any relatives? | What's that? |
| Have you any relatives? | (No answer). |

Throughout this interview the patient gazed fixedly out of the window, often failed to answer questions, and when she did answer, gave brief vague replies with very little content. Six months later she was interviewed by another psychiatrist and the interview showed exactly the same pattern. Many questions were unanswered. Replies were brief and off-putting:

- | | |
|---------------------------------|------------------|
| Q. How long have you been here? | A. A long time. |
| How do you like it? | It's quite nice. |
| Do you want to leave? | Don't know. |
| Would you be content to stay? | Yes. |
| For ever? | Don't know. |
| Have you any relatives? | Which relatives? |
| Would they welcome you home? | Not now. |
| Could you support yourself? | Don't know. |

These extracts give the flavour of most interviews rated (4) on this scale. A male patient rated (4) because of the vagueness, hesitancy and lack of content of his replies spoke as follows:

- | | |
|-----------------------------------|---|
| "How do you like it in hospital?" | "Well, er . . . not quite the same as, er . . . don't know quite how to say it. It isn't the same, being in hospital . . . as, er . . . working. Er . . . the job isn't quite the same, er . . . very much the same but, of course, it isn't exactly the same." |
|-----------------------------------|---|

A patient who constantly murmured under his breath (e.g. Leonhard's *prosectic catatonia*) or who made an occasional random remark, but who made no reply to questions, was rated (5).

Incoherence and Irrelevance of Speech

Incoherence was defined as lack of logical relationship between sentences, or such abnormal construction of sentences that the meaning was unclear. It was rated on the whole interview, including replies to the questions about discharge, etc. (Patients sometimes demonstrated incoherence in answers to questions about delusions but not otherwise.) In practice, there was little difficulty in making this rating. The following is a sample of a conversation with a female patient who was rated (4). She usually began to answer to the point but very rapidly wandered off.

- | | |
|--------------------------|---|
| How do you like it here? | I shouldn't like to stay here too long. I didn't really want to get degraded as mental . . . Of course, my surgeon was Eastern . . . I really |
|--------------------------|---|

wanted a doctor to sign me out on the gratification list. Of course, there was the ensnearing list of the nation . . . My husband would come and accuse a woman. At last it was taken up in our way (etc. etc.).

Six months later, when interviewed by another psychiatrist, she showed the same tendency.

- | | |
|--|---|
| Q. How do you like it? | A. Well, I'm not as settled. I want to raise myself. |
| Q. Do you want to leave? | A. I would like to go to my own home again. |
| Q. Would you be content to stay? | A. No, I wouldn't. My job in the village was to mastertall. I don't want to stay here. I'm not a name at all, really. Our belief is in the heart. |
| Q. What are your chances of settling down outside? | A. I'm a married woman. There's a man here—he's an assistant beast. |

Coherently Expressed Delusions

The questions asked routinely have been listed above. A typical patient rated (4) would give coherent and logical answers to the questions on neutral topics, usually with a hint somewhere of his delusions, but these would only be definitely and fully expressed on direct questioning. A sample of conversation with a patient rated (5) is given below. Her delusions intruded into her replies to neutral questions, but they were expressed with clarity and coherence.

- | | |
|-----------------------------|---|
| Q. How do you like it here? | A. It's nice. But I like to go out with my people. I've got my brothers and my father and mother. But I don't want to live with them. The police don't want me to mix. They want me to be single. I'm well known at Scotland Yard. Because I was crowned 23 years ago—the sea queen of England. I was a princess. The police thought I was going to be married to a common man and they made me break it off. |
|-----------------------------|---|

Six months later, when interviewed by another psychiatrist, the interview followed a similar pattern except that the delusions were only elicited by direct questioning and she was rated (4).

Thus most of these ratings were based on what the patient said during a clinical interview (they are, therefore, behavioural ratings). If he said nothing, or denied delusions when they were in fact present, no correction could be made. Moreover, the ratings represented a cross-section of a process occurring, and sometimes fluctuating, in time. A complete description would call for frankness on the part of the patient, a reasonable sample of conversation, and a longitudinal series of ratings.

PRINCIPLES OF CLASSIFICATION

It was found in preliminary work that severe flatness of affect could co-exist with any combination of ratings on the other three symptoms. It was not,

therefore, of much help in classifying patients, except in one respect which will be mentioned later. There are two natural methods of dividing the ratings of poverty of speech, incoherence of speech and coherent delusions. One is according to severity of the verbal disorder shown. Patients rated (1) on each of the three scales have no evident disorder during the interview. Those rated (2) show no disorder during the interview but may have symptoms at other times. Those rated (3) or less on all three scales may be said to be only moderately disturbed in speech. Patients with ratings of (4) or (5) can be said to be severely, or very severely, disordered. The other natural method of classification is according to which type of verbal symptom is predominant.

The two methods may conveniently be combined to give five subgroups which are fairly easily recognizable clinically, as follows:

- Subgroup 1. Patient with only moderate disturbance of speech (i.e. those rated (3), (2) or (1) on all three scales).
- Subgroup 2. Patients with coherent delusions as the predominant verbal symptom (i.e. a rating of (4) or (5) on coherent delusions).
- Subgroup 3. Patients with incoherence of speech as the predominant verbal symptom (i.e. a rating of (4) or (5) on incoherence of speech. Such a rating takes precedence if, as rarely occurs, a patient is also rated (4) on coherent delusions or poverty of speech).
- Subgroup 4. Patients with a rating of (4) on poverty of speech.
- Subgroup 5. Patients with a rating of (5) on poverty of speech (i.e. the mute, or almost mute, patients).

This simple classification gives five subgroups which are mutually exclusive and jointly exhaustive (2).

By using the rating of flatness of affect, subgroup 1 may be further divided into three sections:

- Subgroup 1 (a) Patients rated (1) or (2) on all four scales, i.e. those who show minimal disorder at interview.
- Subgroup 1 (b) Patients rated (3) on any of the four scales, i.e. those who demonstrate at least one of the symptoms in moderate degree.
- Subgroup 1 (c) Patients rated (4) on flatness of affect, and (3), (2) or (1) on the other three scales, i.e. a borderline group intermediate between subgroup 1 (b) and the more severely handicapped subgroups.

CRITERIA OF DIAGNOSIS OF PATIENTS STUDIED

All the patients studied had been in hospital for at least two years, and were under 60 years of age. Patients from three mental hospitals, in different parts of the country, were interviewed. (These hospitals will be known as hospitals A, B and C.) The case records of all patients described on the front sheet as schizophrenic were examined. Patients who had been assessed as feeble-minded during their schooldays, or who had a history of epilepsy or organic cerebral disease before the onset of illness, or in whom there was clear evidence of obsessional phenomena, were excluded. The remaining records were examined for definite evidence of delusions, schizophrenic thought disorder, or catatonic motor disturbances at some time during the history. If no one of these was present, the patient was excluded. At interview, a further check was made on doubtful cases where the symptomatology was not entirely clear from the case-record. Persecutory delusions, in the absence of guilt and depression, were accepted as sufficient evidence for inclusion even when ideas of influence and control were not

present. Predominantly affective disorders were excluded. These criteria, in conjunction with the limitation that all patients had been in hospital for over two years, led to a fairly strict selection of cases for inclusion in the study.

RELIABILITY OF THE CLASSIFICATION

In order to determine the limits within which chronic schizophrenics selected in this way can be reliably classified, two separate estimations were made: one under conditions in which reliability was likely to be relatively low, and one under conditions likely to yield a relatively high reliability. On each occasion, 50 chronic schizophrenic patients who had been in Hospital A for two years or more, and who were under 60 years of age, were interviewed by the investigator and a colleague (Dr. A. Catterson).

In the first instance, the investigator rated 50 female patients in March, 1960, and they were re-rated 6 months later by Dr. Catterson. At that time the second rater had not used the scales before, and he was given no more explanation than is printed in the Appendix. He had been working on the female side of Hospital A for one year and had a much closer knowledge of the patients in the sample than had the investigator. Dr. Catterson received his psychiatric training in Canada. Thus the patients might well have changed in symptomatology during the six months between ratings, and the interpretations of the verbal material recorded at the standard interview, as well as the technique of interviewing, might well have been different. In this way, conditions were created in which agreement on classification was likely to be minimal. The correlation coefficients between the two interviewers' ratings on the four scales were indeed low ($r=0.56, 0.55, 0.71$ and 0.49). The degree of concordance in classification is shown in Table I. The two raters agreed on 72 per cent. of cases. When subgroups 1 and 2, and subgroups 4 and 5, are combined, $\chi^2=67.7, p<.001$. The contingency coefficient is 0.76 (the maximum value of this coefficient for a 3×3 table is 0.82).

TABLE I

Five-part Classification of 50 Chronic Schizophrenic Women Based on Ratings made by Two Psychiatrists, with an Interval of Six Months between Ratings

First Rater's Classification					Second Rater's Classification				
					1	2	3	4	5
1. Moderately ill	..	..	..	..	16	—	—	—	—
2. Coherent delusions	..	..	..	..	4	3	—	—	—
3. Incoherence of speech ..	..	..	..	..	—	—	9	—	—
4. Poverty of speech	..	..	..	..	4	2	2	5	—
5. Muteness	..	..	..	..	—	—	1	1	3

During this exercise, the second interviewer obtained considerable experience in carrying out the standard interview and using the scales. In addition, the protocols of all interviews about which there was disagreement were discussed in detail and a mutually acceptable assessment decided upon. A further series of 50 chronic schizophrenic patients from Hospital A (25 men and 25 women) were then seen by both raters together, each conducting the interview alternately. The danger of contamination through leading questions (suggesting which rating the interviewer was going to make) was recognized. Such questions would in any case be difficult to devise in the context of a standard interview, but a deliberate effort was made to avoid them. The ratings were made immediately at the conclusion of each interview before any verbal exchange was made between raters.

Under these conditions, the reliability coefficients on the four scales were 0·78, 0·86, 0·93 and 0·85. The degree of concordance in classification is shown in Table II. Eighty-four per cent. of the cases were classified in the same way by the two raters.

TABLE II

Five-part Classification of 50 Chronic Schizophrenic Men and Women Based on Ratings made by Two Psychiatrists at the Same Interview

First Rater's Classification	Second Rater's Classification				
	1	2	3	4	5
1. Moderately ill	18	1	—	3	—
2. Coherent delusions	2	6	—	—	—
3. Incoherence of speech	—	—	3	—	—
4. Poverty of speech	2	—	—	10	—
5. Muteness	—	—	—	—	5

VALIDITY OF THE CLASSIFICATION

The validity of the classification can be determined in a number of ways, depending on the use to which it will be put. One important method is to measure independently the ward behaviour of the patients classified, in order to show that the different subgroups behave in significantly different ways. A behaviour rating schedule (form 1) had previously been constructed for the purpose of measuring change in the behaviour of moderately handicapped male chronic schizophrenic patients during courses of rehabilitation. When the statistical relationships between the items were investigated by means of factor analysis, in two separate samples of chronic schizophrenic men, it was clear that two subtotals could be used. One subtotal represented "Social Withdrawal" and the other "Socially Embarrassing Behaviour". Both were consistent and reliable. These scores differentiated significantly between three clinical subgroups of patients (8). For the present study, a simpler 12-item scale (form 2) was constructed which would be suitable for all grades of patient and which could be administered on a large scale. The items are presented in Appendix B. One hundred schizophrenic women, randomly selected from all those in Hospital C who had been resident for more than two years and who were aged 59 or under, were rated on these items by their ward sisters. The matrix of inter-correlations between the ratings on 11 of the items was calculated and a principal components analysis carried out. Item 3 was omitted because of infrequency of rating. Five components were extracted of which the first three are shown in Table III.

TABLE III

Principal Components Analysis of Behaviour Rating Scale

Direction of Scoring						Factor I	Factor II	Factor III
1. Slow	..	..	..	..	..	·800	·020	—·293
2. Underactive	..	..	..	..	..	·856	·008	—·191
3. Overactive*	..	..	..	..	..	—	—	—
4. Lack of conversation	..	..	..	..	..	·805	—·207	—·066
5. Social withdrawal	..	..	..	..	..	·740	·095	·020
6. Indifference	..	..	..	..	..	·810	—·107	—·101
7. Talking to self	..	..	..	..	..	·122	·775	·395
8. Mannerisms	..	..	..	..	..	·483	·571	—·435
9. Threats of violence	..	..	..	..	..	·387	·687	·226
10. Poor personal hygiene	..	..	..	..	..	·778	—·166	·062
11. Careless of appearance	..	..	..	..	..	·782	—·128	·334
12. Poor mealtime behaviour	..	..	..	..	..	·626	—·395	·452

* Item 3 omitted because of infrequency of rating.

The first factor has high loadings on slowness, underactivity, lack of conversation, social withdrawal, indifference, poor personal hygiene, careless appearance and poor mealtime behaviour. In the analysis of form 1 of the schedule, the first factor extracted had high loadings on almost identical items. This factor was called "Social Withdrawal". It clearly embraces a wider range of behaviour but, for convenience, the letters S.W. will be used to designate this factor.

Factor II has high loadings on talking to self, mannerisms and threats of violence. In the analysis of form 1 of the schedule these items also had high loadings on the second factor, as did overactivity and abnormal speech content. This factor was called "Socially Embarrassing Behaviour" and the letters S.E. will be used to designate it.

The remaining three components which were extracted do not lend themselves to interpretation, and the loadings are mainly negligible. This was the case with factor III in the previous work. The correlation between S.W. and S.E. in the earlier study was 0.29 and 0.19, in two different series of ratings. In the present work, it was 0.31 in a series of 273 chronic schizophrenic women from Hospitals A, B and C.

RELIABILITY OF THE BEHAVIOUR RATING SCALE

A charge nurse on one of the wards in Hospital A was asked to rate 24 severely ill male patients, whom he knew well, every week during a 14-week period. During the first 8 weeks a second charge nurse also completed the schedule, and during the final 5 weeks a third charge nurse completed it. The ratings were made independently and none of the nurses kept a record of his ratings. There was no period of preliminary training. Correlation coefficients were calculated between pairs of nurses' ratings, and between sets of ratings over time. These are set out in Table IV. The S.W. score is reliable between raters and over time, whereas the S.E. score is not always reliable between raters, and is variable over long periods of time. These findings, again, are very similar to those with form 1 of the schedule.

TABLE IV

Reliability of Social Behaviour Scores, between Raters, and Over Time

Charge Nurse(s)	Time Interval	Correlation Coefficient	
		S.W.	S.E.
X and Y	Same week	87	.71
X and Y	Same week	91	.80
X and Z	Same week	77	.36
X and Z	Same week	72	.51
Y and Z	3 weeks between ratings	74	.74
X	1 week between ratings	90	.93
X	14 weeks between ratings	83	.16
Y	5 weeks between ratings	61	.83
Z	5 weeks between ratings	79	.76

RELATIONSHIP BETWEEN MENTAL STATE AND BEHAVIOUR

Data from surveys of the female chronic schizophrenic populations of three mental hospitals were used to demonstrate the relationship between mental state classification, and ward behaviour independently rated by the ward sister. In Hospitals A and B, random samples (stratified by wards), of 100 female

patients who had been in hospital for more than two years and who were aged 59 or less, were interviewed by the investigator. In Hospital C, all the 73 patients satisfying these criteria were interviewed. There was no possibility of contamination between the interviewer's and the ward sisters' ratings. The patients were divided into five clinical subgroups by the method described, and the mean score on each behavioural item was calculated for each subgroup. These results are shown in Table V.

TABLE V

Mean Score on Each Item of the Behaviour Rating Scale for Five Clinical Groups

(273 female chronic schizophrenics from 3 mental hospitals)

S.W. Scale:	Item	Clinical Group				
		1	2	3	4	5
1. Slow		0.25	0.41	0.57	0.56	1.00
2. Underactive		0.16	0.28	0.55	0.54	1.18
4. Lack of conversation		0.26	0.31	0.55	0.80	1.53
5. Social withdrawal		0.45	0.52	1.02	1.03	1.63
6. Indifference		0.30	0.41	0.91	0.90	1.55
10. Poor personal hygiene		0.07	0.14	0.29	0.46	1.10
11. Careless of appearance		0.16	0.21	0.52	0.70	1.38
12. Poor mealtime behaviour		0.03	0.10	0.12	0.30	0.70
<i>S.E. Scale:</i>						
3. Overactive		0.10	0.31	0.31	0.33	0.23
7. Talking to self		0.29	0.66	1.10	0.73	0.65
8. Mannerisms		0.16	0.35	0.50	0.51	0.70
9. Threats of violence		0.22	0.38	0.43	0.37	0.43

Subgroup 1 was further divided into three sections, and the mean S.W. and S.E. scores for these, and the other four subgroups, are shown in Table VI. The two subgroups of patients who showed severe "florid" symptoms at interview (coherent delusions or incoherence of speech) are shown separately from the rest.

TABLE VI

Mean Behavioural Scores of 273 Patients in Five Clinical Subgroups

Clinical Subgroup							N	%	Mean Score		
									S.W.	S.E.	
<i>Exhibiting severe florid symptoms:</i>											
Subgroup 2	Coherent delusions					..	29	10.6	2.38	1.70	
Subgroup 3	Incoherence of speech					..	42	15.4	4.53	2.34	
<i>Without severe florid symptoms:</i>											
Subgroup 1(a)	Minimal symptoms					..	29	10.6	0.96	0.61	
Subgroup 1(b)	Moderate symptoms					..	48	17.6	1.79	0.75	
Subgroup 1(c)	Moderate verbal disorder but severe flatness of affect						15	5.5	3.39	1.20	
Subgroup 4	Poverty of speech						70	25.6	5.29	1.94	
Subgroup 5	Mute						40	14.7	10.07	2.01	
Total	..	..	..	..	..	..	273	100.0	4.27	1.59	

Analysis of variance discloses a significant degree of variation between the five subgroups on both the S.W. and the S.E. scores. There is an obvious gradation in both scores, in the patients without severe florid symptoms, from those in subgroup 1(a), who show minimal disorder at interview, to those in subgroup 5, who are quite mute. There is also a significant difference between the two groups showing severe florid symptomatology, on each of the behavioural scores.

The distinctions between subgroups 2 and 3, on the one hand, and subgroups 1, 4 and 5 on the other, cannot be made in terms of these two scores. However, an independent check was made by using two other published ward behaviour scales. Venables (6) has constructed an "activity-withdrawal" scale, and Venables and O'Connor (7) have designed a scale which yields a score representing the patient's position on a "Paranoid-Non-paranoid" continuum. If appropriate cut-off points are chosen (15 and below representing "Withdrawn", 5 and above representing "Paranoid"), four categories are created which can be compared with the subgroups of the present classification (combining subgroups 4 and 5). The scores of all the 57 male schizophrenic patients in Hospital A, who had been in hospital over two years, were aged 59 years or less and were not receiving medication, were made available by Dr. Venables, and an independent classification made by the investigator. The results are shown in Table VII. There is a considerable degree of concordance between the two methods of classification ($C=0.74$).

TABLE VII
Comparison of Two Systems of Classification

Activity-Withdrawal and Paranoid-Nonparanoid Scales				Present Classification			
				Subgroup			
				1	2	3	4 and 5
Active, nonparanoid	..	..	..	9	—	2	1
Active, paranoid	..	..	..	1	13	2	2
Withdrawn, paranoid	..	..	..	1	—	10	4
Withdrawn, nonparanoid	..	..	..	2	1	1	8

CLINICAL INTERPRETATION OF THE SUBGROUPS

Since ratings of the four symptom scales were reasonably reliable as between two psychiatrists who had had considerable training, the ratings can be used to classify patients in other ways. During the extensive survey of chronic schizophrenic patients in three mental hospitals, which has already been referred to, an attempt was made to make a clinical classification, so that the research categorization could also be studied in descriptive clinical terms. Considerable difficulty was experienced in placing patients into discrete clinical subgroups, even using Leonhard's detailed guide (4). The exercise served to illustrate the complexity of the problems involved, and to emphasize the value of a system of rating which does not assume that symptoms are mutually exclusive.

Subgroup 1a. None of the four symptoms could be elicited at interview with 29 patients out of 273, so that no comparison with other subgroups is available. These patients would all come within Leonhard's non-systematic schizophrenias.

Subgroup 1b. Thirty-nine out of 48 patients in this category showed moderate blunting of affect with or without some poverty of speech. The remaining 9 patients also showed moderate incoherence or fragmentary delusions. Kraepelin's description of "simple weak-mindedness" (3) covers most of the defects seen in this group.

Subgroup 1c. The 15 patients in this subgroup were similar to those in subgroup 1b except that they all showed severe flattening of affect. Six also had moderate florid symptoms (incoherence of speech or coherent delusions). Most were clinically classified as shallow, silly or eccentric hebephrenics.

Subgroup 2. Twenty-two of the 29 patients in this subgroup showed coherent delusions without incoherence of speech. Indeed, their speech was often pedantically correct. They were clinically classified into Leonhard's phonemic, expansive or confabulatory paraphrenic groups. Seven patients also showed moderate incoherence of speech—these were mainly fantastic, hypochondriacal, or phonemic paraphrenics.

Subgroup 3. Twenty-two of the 42 patients in subgroup 3 were also fantastic or hypochondriacal paraphrenics who presented with severe incoherence of speech but who nevertheless showed evidence of persisting delusions. Eleven patients were closer to Leonhard's category of schizophasia. The remaining 9 patients were proskinetik catatonics who muttered on incoherently but audibly after answering a question.

Subgroup 4. There were 21 patients, out of 70 in this subgroup, who showed moderate florid symptoms—most of them proskinetik catatonics. The remaining patients restricted themselves to very brief answers and were placed into Leonhard's categories of speech-poor catatonias, or into autistic hebephrenia.

Subgroup 5. Most patients in this group were members of Leonhard's category of speech-inactive catatonia. A few could be placed into the categories of negativistic catatonia, rigid catatonia, or incoherent paraphrenia.

Thus it is clearly possible to divide subgroups 2, 3 and 4 further, according to whether the predominant symptom is shown in relatively pure form, or combined with another symptom, and new groups could be formed by combination. There is little point in such an exercise unless it can be shown that objective measurements discriminate between such subgroups. However, this rating scheme does provide opportunity for extensive re-classification.

DISCUSSION

The two assessments of the reliability of this simple 5-part classification show that it can be used consistently within a reasonable range of experimental conditions. Even when one interviewer had had no experience of the technique and there was an interval of six months between interviews, only 14 patients out of 50 were placed into different categories. This result gives support to Leonhard's contention that the leading symptom in chronic schizophrenic conditions does not show much change once the patient has been ill for 10 years or more. When both interviewers were practised in the method, and the patients were seen at the same time, the number of disagreements dropped to 8 out of 50. Reliability of this kind could be obtained only by rigorously limiting and standardizing the information obtained.

The classification is suitable for schizophrenic patients, diagnosed according to strict criteria, who have been in hospital for two years and are aged under 60. Whether it would be appropriate for other categories of patient has not been assessed—it is doubtful whether it could be usefully applied to acute schizophrenic psychoses.

The five subgroups can also be distinguished by scores derived from nurses' ratings of patients' behaviour in the ward, if a distinction is made between patients who show severe florid symptoms at interview (coherent delusions and incoherence of speech) and the rest.

None of the subgroups defined in this classification is homogeneous. The moderately ill patients may be passing through a phase of remission and may later relapse: or they may be concealing coherent delusions: or they may always have had a mild form of the illness. Patients with coherent delusions may be subdivided in a number of ways according to content. There may be a number of different forms of incoherence of speech, and patients with this symptom may sometimes speak very little so that they are classified in subgroup 4. Mute patients do not always remain mute, and they may be silent for various reasons. These objections are sufficient to preclude the use of the classification for clinical purposes, unless other information is also taken into account. Moreover, the various categories may not correspond to aetiological entities, and are likely to be supplanted by more fundamental measurements. However, as a method for comparing and describing populations and for the study of psychological and social differences between subgroups, the classification has fewer limitations than presently existing clinical systems, and there is the further advantage that the system of ratings can be classified in other ways if necessary. The utility of the distinction between moderately and severely handicapped patients has already been established in a rehabilitation experiment which showed a markedly different success rate in the two categories (9). Other uses are being determined by current experimentation.

SUMMARY

A simple classification of chronic schizophrenia, based on ratings of mental symptoms made at a standard interview, has been shown to have satisfactory reliability as between raters, and to be relatively stable over time. Scores representing different types of social behaviour provide some validity for the classification.

APPENDIX A

Scales for Rating Four Schizophrenic Symptoms

1. *Flatness and Incongruity of Affect*

1. No evidence.
2. Indirect evidence only (e.g. case notes, nurse's report, etc.).
3. Occasional episode of definite flatness or incongruity, but mainly appropriate affect.
4. Affect mostly inappropriate or flat, but occasionally appropriate.
5. Complete flattening. No affect unless incongruous.

2. *Poverty of Speech*

1. No evidence.
2. Indirect evidence only (e.g. case notes, nurse's report, etc.).
3. Definite vagueness, stereotypy, repetitiveness or wandering, but interview relatively intact.
4. So vague, wandering, repetitive or stereotyped, that interview almost impossible.
5. Mute or almost mute.

3. *Incoherence of Speech*

1. No evidence.
2. Indirect evidence only (e.g. case notes, nurse's report).
3. Definite incoherence, but rest of interview little affected.
4. Definite incoherence, interfering markedly with rest of interview.
5. Practically nothing coherent.

4. *Coherent Delusions*

1. No pre-occupation evident.
2. Indirect evidence only (e.g. case notes, nurse's report, marked evasion).
3. Some evidence of coherently expressed delusions, but these have little force now. Little active pre-occupation.
4. Evident active pre-occupation, but can give attention to other matters.
5. Can hardly attend to anything else.

N.B.—Detailed transcripts of interviews with patients who were characteristic of the various subgroups are available on request.

APPENDIX B

Please consider this patient's behaviour during the past week only, even if it was not typical of his or her usual condition.

There are three items in each section. If one of the items describes behaviour which has occurred in the past week, please place a tick against it in the column on the right. There should be only one tick for each section. Please read all three items before making your choice.

Item 1: Slowness of Movement

- (2) Usually extremely slow to move, e.g. took very much longer over a meal, or dressing, or walking across the ward, than other patients ☐
- (1) Showed periods of extreme slowness of movement as in (2), but at other times was not slow to move ☐
- (0) Speed of movement normal ☐

Item 2: Underactivity

- (2) Stood or sat in one place all the time, with little movement. Even with encouragement was very difficult to get moving ☐
- (1) Showed periods of extreme underactivity as in (2), but at other times was not underactive ☐
- (0) Showed no marked underactivity ☐

Item 3: Overactivity

- (2) Usually extremely overactive or restless, e.g. paced rapidly up and down, became excited, talked or sang loudly or wildly, etc. ☐
- (1) Showed periods of extreme overactivity as in (2), but at other times was not overactive ☐
- (0) Showed no marked overactivity ☐

Item 4: Conversation

- (2) Was mute or almost mute ☐
- (1) Said a few words, e.g. in reply to questions, but was usually silent ☐
- (0) Ordinary conversation ☐

Item 5: Social Withdrawal

- (2) Never mixed socially with anyone, even when encouraged to do so ☐
- (1) Was socially withdrawn and solitary, but would mix a little with others if encouraged to do so. ☐
- (0) Normal social mixing ☐

Item 6: Leisure Interests

- (2) Showed no interest in anything. Did not watch television, read newspapers, play games, etc., even when encouraged to do so ☐
- (1) Showed very little interest, but could be persuaded to watch television, read papers, join in games, etc., for a while ☐
- (0) Showed normal spontaneous interests ☐

Item 7: Laughing and Talking to Self

- (2) Frequent episodes (once a day or more often) of laughing or talking out loud —not just constant smiling ☐
- (1) Occasional episodes of laughing or talking out loud, but these did not occur every day ☐
- (0) No such episodes noted ☐

Item 8: Posturing and Mannerisms

- (2) Adopted odd or uncomfortable postures, or made bizarre movements, every day ☐
- (1) Behaved as in (2), but less often than every day ☐
- (0) No such behaviour seen ☐

Item 9: Threatening or Violent Behaviour

- (2) Struck some person, or destroyed some article (e.g. clothing, window, crockery, etc.), during the week ☐
- (1) Was threatening in manner, or verbally abusive, but did not strike anyone ☐
- (0) No such behaviour seen ☐

Item 10: Personal Hygiene

- (2) Was incontinent on at least one occasion during the week ☐
- (1) Needed raising at night, or escorting to lavatory during day, in case of incontinence, but was not actually incontinent when this was done ☐
- (0) Needed no escorting or raising and was not incontinent ☐

Item 11: Personal Appearance

- (2) Needed to be shaved (if male), washed or dressed fully at least once during the week ☐
- (1) Could shave, dress and wash, but needed supervision with tie, buttons, etc., or would be slovenly in appearance ☐
- (0) Needed no supervision of this kind. Maintained reasonably neat appearance without prompting ☐

Item 12: Behaviour at Meal-Times

- (2) Needed spoon-feeding at least once during the week ☐
- (1) Did not require spoon-feeding, but had to wear bib, or needed supervision because of faulty table manners ☐
- (0) Normal behaviour at meal-times ☐

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THE RELIABILITY OF PSYCHIATRIC DIAGNOSIS

By

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INTRODUCTION

No observation can be incorporated satisfactorily in a body of knowledge unless its reliability, however defined, is also known. In psychiatry, the concept of reliability has in great measure been transplanted from clinical psychology, but its optimal application to meet psychiatric needs requires some attention.

For the purposes of psychiatry, reliability can conveniently be measured as the amount of agreement between different observers examining the same subjects (or a comparable series of subjects). The results may be expressed as the percentage of subjects in which agreement on the relevant variables is reached, or, in the case of two or more closely matched series being employed, by comparing the reported incidence of the phenomena under study. In one or other form this simple principle has been applied in a number of reliability studies, of which the majority have been concerned with the vital question of the reliability of nosological diagnosis (Ash, 1949; Mehlman, 1952; Hunt, Wittson and Hunt, 1953; Seeman, 1953; Foulds, 1955; Schmidt and Fonda, 1956; Pasamanick, Dinitz and Lefton, 1959), while Reid and Finnesinger (1952) have contributed an interesting semantic analysis of the concepts involved.

It emerges quite clearly from these studies that certain factors must be controlled before the findings of one author can usefully be compared with any other. Apart from a brief review by Reid (1960) no attempt seems to have been made hitherto to formulate the nature and relationship of these different variables, although they are not particularly obscure, and it may be worth attempting a systematic examination of them before considering in detail the results obtained by the workers quoted above.

It is suggested that the variables fall into five principal groups.

1. *Variables Relating to the Psychiatrists*

The psychiatrists involved in the study should be "representative", i.e. their frame of reference should be that of general clinical psychiatry, or, if the study is concerned with agreement among those belonging to special schools, this should be explicitly stated. Each of the psychiatrists should be of approximately equal experience, unless, indeed, it is the effect of seniority on concordance values which is under analysis. Further, all should be of reasonable seniority: it is difficult to know what to make of studies which compare the performance of trainees with University teachers, or report on the agreement among medical students (*v. infra*). Apart from considerations of experience, more precise control of other variables relating to psychiatrists may be difficult to obtain in practice, nor has the importance of doing so yet been demonstrated empirically, although there is considerable evidence to show that personality traits in psychiatrists are associated with different kinds of orientation (Kreitman, 1959).

2. *Variables Relating to Psychiatric Examination*

It is necessary to determine and to report explicitly the aims, scope and circumstances of the examinations of the patients. Psychiatric interviews vary a great deal according to the purposes for which they are being conducted, e.g. screening interviews for the Armed Services, as compared with assessment for psychotherapy, or even in-patients' as against out-patients' examinations. Furthermore, not only are there differences in the kinds of conclusions it is hoped to draw from the interview, but the amount of time available for reaching them must differ markedly. Thus a decision is required at the outset whether it is desired to investigate the reliability obtained in the conditions of everyday practice, in an academic setting, or some other situation. Agreement values obtained under such different circumstances may well reflect those differences, or, if the obtained values are the same, may require quite diverse interpretation, nor is it easy to extrapolate agreement levels on the one from the other. It is possible that the more leisurely and detailed an examination the more two different observers are likely to discover similar features in the patient, but it is equally possible that accumulation of detail would lead to increasingly divergent views and diagnostic emphasis. It goes without saying that the same examination conditions should apply to all the investigators, as should the amount of additional information available in the form of test results and special reports, unless of course, in a suitably designed investigation, it is the effect of such differences which is at issue.

It is also evident that the time elapsing between the first and second examination should be as short as possible in order to minimize non-error variance due to fluctuation in the patient's state. In some studies the patient has been interviewed by two or more psychiatrists simultaneously. This has certain administrative advantages and ensures that each doctor is seeing the patient at the same stage of his illness, but it is liable to lead to contamination of judgment and may well alter the character of the patient's responses to questions. At the least it may be said that to generalize from this situation to the customary one-doctor setting is somewhat suspect. It should also be noted in passing that even when the patient is interviewed on two separate occasions his behaviour the second time is liable to differ from the first in many respects. Being largely unavoidable, these differences must be measured or controlled by a suitable experimental design. In the same context may be mentioned the importance of controlling at what stage in the treatment the patient is being assessed, the principal distinction here lying between initial and final diagnosis.

3. *Variables Relating to Nomenclature etc. and Reporting*

It is in this area that the greatest, and most familiar, difficulties arise. It is patently desirable that all investigators participating in a study should employ the same *nomenclature*, using the same, clearly definable terms when referring to the same phenomena or concepts. The problem here is the choice of labels, and although theoretically simple, its practical importance can scarcely be over-estimated. In an ideal situation, there would also exist uniformity of *concept*, such that various phenomena would be generally agreed to represent particular symptoms, syndromes or diseases. Lastly there arises the further question of the principles by which phenomena or diseases should be arranged in a *classification* (Stengel, 1960a). Moreover as the same author has pointed out (1960b) these three considerations are not as interdependent as might at first be supposed. Unless the examiners are agreed on all these points, it can by no

means be inferred that any differences between them are due to "observer error", i.e. genuine disagreements as to what they observe and elicit from the patient.

Some, but not all, of these difficulties can be met by prior agreement among the psychiatrists concerned. Certainly one may justifiably be suspicious of semantic confusion in reliability studies in which this step has been omitted, as for example, in those based on comparisons of findings from different centres, unless, of course, it is desired to examine this issue specifically. At the same time it must be remembered that any study utilizing a terminology which differs radically from those in common use is likely to be of local interest only, and further, that with the best will in the world it is very difficult for the participants in an investigation consistently to apply psychiatric terms in a manner markedly divergent from their familiar, if vague, usage. So far as uniformity of classification is concerned, it might be possible by prolonged discussion among the participants to establish harmony, but it must then be appreciated that the agreement values obtained will probably be inapplicable to judgments made in more mundane circumstances. The optimal and most feasible course would appear to lie mid-way between using an unfamiliar set of definitions, artificially agreed upon, and the everyday confusional state of psychiatric parlance. Preliminary discussion may clarify nomenclature, while leaving the subtler differences of concepts.

Another point arises here. Conceptual clarity does not follow automatically the choosing of adequate operational criteria for psychiatric terms. It is a question of discovering definitions which are both clear-cut *and* comprehensive enough to embrace the needs of the clinical situation. Thus a "previous illness" may be defined for epidemiological purposes as a previous referral to a psychiatrist or previous admission to hospital. Neither definition would satisfy the clinician, who may legitimately wish to include a history of untreated earlier illness. It may therefore be necessary to assess the reliability of examination separately for each context or definition. The reliability estimates obtained will probably differ, since in effect they relate to different things. In the epidemiological instance the question is "With what consistency of results can a certain well-defined criterion be applied?" In the clinical situation it is "How much do clinicians agree when using criteria which they find useful for their own purposes major semantic discrepancies having been eliminated so far as is practicable?"

Similarly, it is not possible to standardize the detailed procedure of a general psychiatric assessment, particularly under normal working conditions, though it is quite feasible for a group of psychiatrists to agree on the main rubrics under which they will set out their findings. The existing literature has little to say on this matter, and there is a marked lack of any attempt to isolate those aspects of examination which contribute most to inter-observer discrepancies.

4. *Variables Relating to the Patients*

It has already been intimated that the population of subjects examined must not only be explicitly described but should comprise a group of patients characteristic of those commonly seen by psychiatrists. The relevant variables in this context would appear to be the range of diagnoses represented, the severity of illness and the socio-economic background. Of these the first-mentioned is of particular importance, as the existing literature makes clear. Reference has already been made to the danger of generalizing the findings

obtained from special groups, as for example, those who are predominantly free from psychiatric abnormality, as in Service screening procedures, or children, or patients seen in general hospitals. Assessment of the reliability which can be obtained with these different subjects would be an interesting study in itself but inferences from such studies to the situation which pertains in everyday psychiatric work would be fraught with difficulties. If the comparative incidence approach is being used, some clear validating method—such as a final diagnosis, jointly arrived at, on discharge or follow up—should ideally be established to assess the “true” incidence of the various diagnoses, in order that the series can be established as comparable. Failing this, careful control of the age, sex, social and similar characteristics of the series would seem to be the only alternative. This also implies the use of large series (but see below).

5. *Manner of Analysis*

Most studies of diagnostic reliability report their results in terms of percentage agreement. This is perfectly satisfactory, provided it is made clear what ratios are being employed. But confusion often arises as Ash (1949) and others have pointed out, when the question of “statistical significance” is introduced. Clearly, if two psychiatrists examine a given patient and may use any one of ten diagnoses, then the probability of their concurring on a basis of random choice is 10 per cent. If, therefore, they examine 100 patients and reach the same diagnosis in 25, they are doing much better than would be predicted by chance alone, and their performance will be very highly “significant”. Far from being a source of satisfaction on this account, such a result would be little short of catastrophic from the viewpoint of clinical standards. In practice, moreover, the position is further complicated in that not all diagnoses are equally probable among the patient population, as Foulds (1955) and many other authors have stressed. In short, it is scarcely ever justified with questions of this kind to employ statistics involving the theory of chance or random effects, and where these are legitimately employed it must constantly be remembered that the level of confidence with which it may be said that a demonstrated association differs from chance expectation is a poor clue to its importance.

Sometimes an analogous situation is found in data relating to the incidence of different disorders. Thus suppose two neighbouring hospitals published figures of the incidence of a certain disorder among their patients. If each hospital has 2,000 patients and the first gives a figure of 965 while the second reports 1,035, these proportions differ at the .05 level of probability. Yet the discrepancy between them is only $3\frac{1}{2}$ per cent., and probably of little importance for all practical purposes.

It must also be borne in mind that statistical artefacts are likely to arise by the use of categorical classifications, as already indicated. Two doctors may well describe a given patient in terms which are almost identical, yet the differences in emphasis may lead to the patient being classified under separate headings, exactly as if the disagreement were a substantial one. Thus one psychiatrist may regard a patient as having a mild chronic schizophrenic illness while the second reports a schizoid personality disorder. These diagnoses are close but not identical: the former would have to be classified as a case of schizophrenia, and further, as a functional psychosis, while the second would probably be grouped with personality disorders, exactly as if the first psychiatrist

had diagnosed acute paranoid schizophrenia while the second had regarded the patient as a fetishist. These difficulties can sometimes be overcome by marshalling the data under broader diagnostic categories, although in the example given this would probably not assist materially. For this reason, it has often been pointed out that the agreement levels reported in the literature tend to be lower than the situation would warrant. Foulds' (1955) attempt to rate disagreement deserves special mention in this context.

PREVIOUS RESULTS

With so many variables haphazardly controlled, it is not surprising that there is little unanimity of results in the literature, nor is it easy to decide where the reliability levels reflect genuine disagreement among the psychiatrists, inadequacy of experimental design or unworkable classificatory systems.

The principal studies using the "paired-opinion method" are summarized in Table I, in which "category diagnosis" refers to major groupings such as "organic diseases" or "functional psychoses", while "specific diagnosis" refers to the precise type of illness, such as "obsessional neurosis" or "hebephrenic schizophrenia."

The outstanding and by far the largest study to date is that of Norris (1959) who reported on a series of about 6,000 patients seen initially in observation units and subsequently admitted to one of two mental hospitals or an associated geriatric centre. She is able to report agreement values on two main series, with sub-divisions by age and sex. The categories are based on the diagnoses made in the observation units, while the percentages refer to those patients who were similarly diagnosed at the mental hospitals. The study is particularly valuable in that, where disagreements in diagnosis have arisen, full details are given of the second diagnosis, so that it has been possible to calculate from the author's data the general findings represented in Table I. It should be noted that the time between examinations was often long enough to permit considerable change in the patients' state, especially as admission, when the first examination was made, usually occurred at the onset of a florid illness. It is probable that the staff at the hospital had fuller data than was available at the observation unit, including the notes made at the latter centre. It is also unfortunate for the present purpose that the data on the geriatric cases were often incomplete, and that the author re-classified diagnoses of "reactive depression" made at the hospital as affective disorders to meet the needs of other analyses. Nevertheless, her data on specific diagnoses are by far the most trustworthy yet produced.

The study of Ash (1949) was based on a series seen at a United States Government centre and was composed largely of minor character disorders and of normal subjects on whom personality assessments were made. Interviews were conducted jointly. The percentages refer to the number of patients on whom agreement was reached expressed as a fraction of the total seen by each pair or trio of psychiatrists. The classification used did not employ mutually exclusive categories nor was the diagnostic range of the series such as to cover it adequately.

The remaining studies were based on in-patients. That by Hunt, Wittson and Hunt (1953) compares the diagnoses reached by the staff of a screening station with those made at a psychiatric hospital: the personnel of the latter had access to the notes of the former. The percentages presumably refer to the proportion of patients in each category as assessed by the first centre in which

TABLE I

Summary of Studies of Diagnostic Reliability

Notes

Agreement on Specific Diagnosis
Within Category

Agreement on Category Diagnosis

Interval
Between
InterviewNo. of
Patients

Author

Classifi-
cation
Used

Norris (1959)	2,804 males 3,459 females	2-4 weeks approx.	I.S.C. modified	Functional psychoses	89 %	Schizophrenia 68 % Manic-depressive 69 % Paranoid psychosis 29 % 66 %	Agreement between mental hospital and observation wards (two series here taken together): all acute cases; contamination of opinion; additional data probably available at second centre: values calculated from author's detailed data.
				Neurosis	46%†	Specified organic psychosis 80 % (except arteriosclerotic) 53 % Senile dementia 29 %* Cerebrovascular psychosis 90 % G.P.I. 92 % Epileptic psychosis 63 % Other organic psychoses 83 %	
				Character disorder	44 %	Alcoholic psychoses 64 %	
				Other	31 %	Mental defect 72 %	
Ash (1949)	52 males	Joint	A.P.A.	Average on all major groups: Between pairs of psychiatrists 58-68 % Between three psychiatrists 46 %		Average on all specific diagnoses: Between pairs of psychiatrists 34-43 % Between three psychiatrists 20 %	Joint interview on O.P. series the majority being normals or minor character disorders.
Hunt, Wittson and Hunt (1953)	794 males	Not stated	Not stated	Personality disorder Psychoses Psychoneurosis	74 % 59 % 24 %	Personality Psychoses 46%† Psychoneurosis 31%† Mental defective 12%† Epilepsy 91%† 72%† Average 33%†	Agreement between screening station and psychiatric hospital: contamination of opinions.
				Average	54 %	Average	
Schmidt and Fonda (1956)	426 males and females	3 weeks	Modified A.P.A.	Organic disorders Psychoses Neurotic and personality disorders	92 % 80 % 71 %	Organic disorders 74 % Psychoses 47 % Neurotic and personality disorders 24 % Average 55 %	Agreement between trainees and teachers: more extensive information available for 2nd psychiatrist.
				Average	84 %	Average	
Foulds (1955)	females 36 males and	1 week	Special	Average agreement of 4.11 on 0-6 rating scale (S.D. 1.71-1.84)			I.P. series, acute cases eliminated.

* Partly due to incomplete data.

† Partly due to author's re-classification.

‡ Calculated on data reported by author in another form.

the staff of the hospital concurred. A considerably higher level of agreement was reached on matters such as fitness for service (94 per cent.). The investigation by Schmidt and Fonda (1956) contains in addition to the information tabulated an attempt to apply correlation coefficients to the statistical problems in this field. It is unfortunate both that the second psychiatrist in each pair was so markedly senior to the first and that he had available additional sources of information concerning the patients. Foulds' (1955) study has already been mentioned. He attempted to assess quantitatively the degree of concordance reached by psychiatrists, by using a special rating scale. By a similar technique he also investigated how well clinical psychologists perform in arriving at a diagnosis. The "significance" of the levels obtained in each case he determined by using a "guesser" working from age and sex data alone.

Other workers have used the incidence approach whereby the percentages represented by various diagnostic groups in two or more comparable series are compared. Boisen (1938) commented on the proportions of the various sub-groups of schizophrenia found in all the new schizophrenic patients admitted to the mental hospitals of Massachusetts, Illinois and New York State. The differences were striking. The incidence of catatonic schizophrenia in first-admission schizophrenics ranged from 2.7 per cent. to 30 per cent., while for hebephrenic schizophrenia proportions of 17 per cent. to 45 per cent. were reported. Even within the State of Illinois values as far apart as 11 per cent. and 76 per cent. were returned for hebephrenia at different hospitals.

The more recent study by Passamanick *et al.* (1959) used a number of series of female patients, the homogeneity of which by age, residence, educational status, type of admission and marital position was carefully established and which were all admitted to the same hospital, being allocated at random to three different wards. One of these wards, however, had three different administrators during the time of the study. The authors give great attention to the discrepancies between these three psychiatrists, and demonstrate that great variations exist, especially in the incidence of schizophrenia. But one of the series contains only 48 patients, which may well be supposed to be too small for comparisons of this kind. This supposition is supported by the highly deviant values for nearly all diagnoses in this small group. If one accordingly considers the two larger sub-series, together with the patients admitted to the two wards which enjoyed continuity of administration, four series remain varying in size from 68 to 206 patients, among which the extremes of incidence are as follows: organic disorders 7.3 per cent. to 10.7 per cent.; affective disorders 3.6 per cent. to 8 per cent.; schizophrenia 22.1 per cent. to 29.3 per cent.; psychoneurosis 29.5 per cent. to 44.7 per cent.; character disorders 12 per cent. to 22 per cent.; other disorders 5.3 per cent. to 9.2 per cent. The greatest discrepancies would thus seem to occur in the incidence of psychoneurosis and character disorders but apart from this the differences are not on the whole particularly marked nor on the above data are they statistically significant ($\chi^2=16.155$, $df=15$, $p<.50$).

A few remaining studies of a similar type may be mentioned, if only for completeness. Masserman and Carmichael (1938) found that about 40 of 100 patients seen at follow-up one year after discharge required re-classifying, albeit to a minor degree in many instances. This finding has more relevance to the problem of the validity of psychiatric diagnosis than that of reliability. The same may be said of a series of papers by Hunt and his colleagues (Wittson, Hunt and Stevenson, 1946; Wittson and Hunt, 1951; Hunt, Wittson and Hunt, 1952; Hunt, Wittson and Hunt, 1955). In 1952 Mehlman reported,

with scant detail, two series of about 1,300 patients accumulated by a large number of psychiatrists whose incidence findings differed at various extreme levels of "significance". So far as his data is susceptible to analysis, however, it would appear that the actual discrepancies were in the order of 20 per cent. His findings appear to be based on several years, over which time the A.P.A. classification which was employed in the study had itself changed. Raines and Rohrer (1955) give data on the reliability of personality typing, assessment of defence mechanisms and predicted response to stress in normal subjects. They conclude that on this material psychiatrists have individual diagnostic preferences which reflect their own personality structure. Seeman (1953) reported on the agreement reached by a class of medical students confronted with six patients.

One method of controlling one of the many variables entering into the assessment of reliability is to use case records or other written documents. The early study of Doering and Raymond (1934) is one of the more comprehensive investigations of this type in that attempts were made to measure the reliability of issues other than diagnosis alone; personality traits, aetiological factors, previous history, symptomatology and the role of environmental processes were all considered. It was also possible to assess the "test-retest" reliability for any one psychiatrist, as well as values for inter-observer agreement between large numbers of doctors all considering the same data. It is unfortunate that the results of this study are too loosely organized to permit any firm conclusions. The use of written data to assess reliability on more limited questions has, of course, been widely used, and the studies of Hunt, Arnoff and Cotton (1954) and Hunt (1959) on schizophrenic thought disorder may be quoted simply as examples. On the whole, the technique has the grave defect of excluding all disagreement of observation and inference at the level of the patient-doctor interaction. It must also be mentioned that information considered adequate by one clinician may not be thought so by another employing different diagnostic criteria. On the other hand, by standardizing the data, the method elucidates how far discrepancies spring from differences in concept and classification. Such studies may contribute much to operational research and problems of statistical compilation, but theoretically the results would give an underestimate of the diversity occurring in clinical practice.

The reliability of assessments of a more limited character such as of individual symptoms, personality typing or psychodynamic processes, is too vast to be profitably reviewed here. The only points which perhaps might be raised are that in the present spate of constructing rating scales of various kinds, there is a danger of forgetting that one can pay too high a price for reliability by loss of clinical relevance (Hamburg *et al.*, 1958) and that quite high validity can be obtained by techniques which appear to have low inter-observer reliability (Cartwright and French, 1939).

It may be worth noting in passing that clinical psychologists, testing the same patients by whatever means they choose, seem to succeed no better than psychiatrists in reaching an agreed diagnosis (Foulds, 1955; Goldfarb, 1959).

THE SCOPE AND LIMITATIONS OF RELIABILITY STUDIES

Before discussing what further developments may be fruitful, it might be useful to consider briefly what reliability studies have to offer.

The importance of diagnosis for psychopathology, epidemiology, therapeutic assessment, clinical psychology and administration needs no repeated emphasis here. Even though there is scant evidence that the present state of diagnosis is as uniformly bad as it is often reported to be, there is clearly room for improvement. Such progress must depend not only on diagnostic studies themselves but also upon developments in a number of other fields. The problem of diagnosis is ultimately one of "validity", which implies the use of some other criterion, such as the natural history or pathology of an illness against which the procedures of clinical diagnosis can be checked. Hence the contribution which studies of reliability can make is necessarily limited, nor is the relation of reliability to validity always clear-cut. Thus two observers describing a patient's personality, or offering a psychodynamic formulation of his illness, may each achieve a measure of predictive validity, yet their accounts may overlap only to a limited degree. Here the "reliabilities" are low yet "validity" is high. Nor can it always be supposed that the reliability of a clinical finding is alone sufficient to guarantee its clinical usefulness. It is possible to determine the sex of the patient with almost 100 per cent. reliability, yet such an observation does not contribute very much in the differential diagnosis. Reliability, in fact, may be as high or higher in irrelevant aspects of the examination as in the most crucial. It may be spuriously raised by insensitivity to subtle and fluctuating symptoms, as Chassan (1951) has stressed, or by neglect to enquire properly into the history. A dozen careless psychiatrists may all agree that a patient's marital adjustment is satisfactory solely because they have neglected to ask the important questions. In these instances reliability may be high yet the validity low, and examples such as these can be readily multiplied. It is enough to point out that blind adherence to reliability alone can be misleading whether applied to diagnosis or any other clinical assessment.

On the other hand reliability can sometimes be of major importance. This can perhaps be most immediately demonstrated in the epidemiological field, as discussed below, but it is of no less importance in clinical work. As an example one might take the continued controversy concerning reactive (neurotic) depression. Those who wish to maintain the existence of such a diagnostic entity must first demonstrate that by using specified criteria it is possible to achieve acceptable standards of reliability (a procedure, incidentally, which can be carried out relatively easily). Failure to do so would mean that the category must be discarded or new criteria developed. Even with generally accepted diagnostic concepts, a parallel use of reliability studies may be highly rewarding. Follow-up or other methods of validating diagnosis are not always readily available or applicable, and in their absence the clinician is obliged to fall back on his own experience, common sense and what is, in effect, an intuitive assessment of reliability. The advantages of making such assumptions explicit need not be stressed. By more detailed knowledge of the reliability of the various elements contributing to the diagnosis, it should be possible to isolate areas of confusion and, by using the more reliable, to sharpen the differentiation between nosological groups.

THE PRESENT NEED

Previous reliability studies in the psychiatric field have concentrated on diagnosis almost exclusively, and the best established—if least surprising—findings to date are those of the different reliabilities associated with the major diagnostic groupings. It appears probable, however, that if further progress is

to be made a more flexible approach will be required, one which aims at the empirical elucidation of the variables mentioned at the beginning of this paper, and which is concerned not only with concordance estimates but also with the *sources* of disagreement. Two further points warrant some amplification, namely, the importance of recognizing the *purpose* for which a diagnostic scheme is being employed, and the necessity of attending to aspects of psychiatric assessment *other than diagnosis*.

In connection with the first point it is worth recalling for a moment that to meet the needs of modern therapeutic, social and other interests, diagnosis is being used more extensively than ever before. Its accuracy is constantly being assumed in situations where such assumptions are crucial to all other inferences. Yet a taxonomy is a conceptual tool, not an absolute, and as such can only be used intelligently when adapted to meet the requirements of the individual using it. That the same labels may have different designations is particularly evident when one compares the roles of diagnosis in the epidemiological and clinical fields. As a recent W.H.O. Report (1960) puts it:

"In . . . field studies, the term 'diagnosis' does not have its usual connotation of complete assessment of cases, but refers to a partial or limited assessment adapted to the needs of the surveys."

Moreover, even within the clinical field the very distinction between health and illness varies from worker to worker, as for example between the psychotherapist and the general psychiatrist. Since, moreover, the same nosological scheme may be put to serve a variety of needs, it is important to appreciate that its reliability is likely to vary according to the use to which it is directed and the circumstances in which it is being operated. It has already been mentioned that extrapolation from one situation to another would seem to be a most hazardous procedure. It remains to be urged that assessments of reliability be separately carried out for all the commonly encountered diagnostic situations. Thus, in the epidemiological field, the most urgent general needs appear to be the assessment of the reliability of the figures quoted by different centres for the incidence of various disorders, and of the reliability and comparability of the different classifications employed: cross-cultural studies in particular are urgently required. However, these needs are widely appreciated and it is interesting to note that the W.H.O. Report already mentioned lists reliability studies first in a survey of current epidemiological research requirements.

In the clinical field consideration of the correspondingly deficit in our knowledge leads directly to the second point mentioned earlier, namely, the need for detailed consideration of aspects of psychiatric examination other than diagnosis. The cardinal need is for greater reliability in diagnosis itself, but to that end it is essential to know what aspects of the psychiatric examination have contributed the most reliable data, and which the least, to our diagnostic certainty. Is there any one facet of history taking which, as currently practised by competent psychiatrists, is more liable than another to lead to error? Which symptoms can be assessed the most reliably? Does the present state of diagnostic confusion reflect differences in what is elicited and observed, or does it rather stem from conceptual difficulties? These and many allied questions still await an answer.

In essence then a plea is being made for adequate analysis. Progress cannot come from the contemplation of bald percentages of agreement; it is less valuable to know that so many psychiatrists agree on a particular diagnosis in a given proportion of cases than it is to understand why they disagree on the

remainder. Traditionally in any medical discipline the answers to the questions posed here are found only by the slow accumulation of experience, often without formal exposition. Perhaps it is not premature to urge that a frontal attack on these problems is likely to prove among the more rewarding of present research possibilities.

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THE RELIABILITY OF PSYCHIATRIC ASSESSMENT: AN ANALYSIS

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THIS study arose out of another enquiry currently in progress (Sainsbury and Grad) into the relative merits of community and hospital care of the mentally sick as practised in the Chichester and District Mental Health Service. For this purpose it was clearly important to know how reliable were the psychiatric assessments made on patients referred to the Service. The occasion was seized to carry out a detailed investigation into the reliability of diagnosis and other psychiatric assessments under N.H.S. conditions, with particular reference to why disagreement between psychiatrists sometimes occurs. As described elsewhere (Carse, 1958; Morrissey and Sainsbury, 1959), the Service supplies all the psychiatric requirements of Chichester and district, and thus provides a wide range of patients treated in a variety of settings.

METHOD

The method employed was the paired-opinion technique, whereby a number of patients were interviewed on two occasions, and the assessments on them compared.

The *psychiatrists* taking part consisted of two hospital consultants and one S.H.M.O. (A, B and C) and two research psychiatrists (X and Y). All have been medically qualified for over 10 years and have had over 4 years psychiatric experience. In each instance, A, B or C conducted the first interview with the patient, while either X or Y carried out the second. There were thus six working pairs: AX, BX, CX, AY, BY, and CY. Each pair saw 15 cases, the patients being allocated at random till the numbers were made up.

The initial *examinations* were made under normal working conditions when the patient was first referred to the Service and in no instance had the patient already been seen on that referral by any other member of the staff. It will thus be evident that the primary purpose of the examination was to make a fairly rapid assessment, to reach an initial diagnosis and decide upon an immediate plan of procedure. Beyond what the patient and his relatives could say, the only additional source of information was the referring letter. The second examination, by X or Y, had close regard to the conditions prevailing at the first session, especially as regards the amount of time that had been available to the Service consultant. Once again, no additional data was available, nor, of course, was X or Y aware of what A, B or C had concluded.

The places at which the patients were examined on each occasion are shown in Table I, from which it will be seen that the series is predominantly composed of out-patients and those seen on domiciliary visits according to the usual practice of the Chichester Service. In some instances A, B or C initiated the disposal of the patient before they were seen by X or Y, so that the second interview took place in a different location, but in all 68 (75.5 per cent.) of the patients were interviewed in the same place on each occasion.

We were most anxious not to interfere with the clinical management of the patients, not only on general grounds but also because it was felt that what little might be lost by differences in this respect would be more than offset by the greater "reality" of the procedure followed.

TABLE I
Place of Examination on First and Second Interviews

		First Opinion (A, B or C)				Totals
		Out-Patients	Domi-ciliary Visit	General Hospital	In-Patients	
Second Opinion (X or Y)	Out-patients ..	41	5	—	—	46
	Domiciliary ..	6	19	3	—	28
	General Hospital	—	1	7	—	8
	In-patients ..	—	6	1	1*	8
		47	31	11	1	90

* Urgency order admitted without prior screening.

Every effort was made to arrange for the second interview to be held as soon as possible after the first. The average time elapsing between examinations was 3.5 days, and in no case was it longer than 7 days. It was subsequently discovered, however, that intervals had not been equal for all the six pairs of psychiatrists, the difference being just significant at the 5 per cent. level. This will be referred to again.

Similarly, where an informant was available at the first interview he was also seen whenever possible on the second. In 25 cases the same informant was seen on both occasions, in 41 cases no informant was available for either examination, while in the remaining 24 different informants were seen, or attended at one interview only. The same situation as regards informants was attained in 66 cases (73.3 per cent.).

The *recording of interviews* and the *nomenclature* employed were determined with some care beforehand. At preliminary discussions between the psychiatrists concerned an attempt was made to eliminate major semantic difficulties and to devise a generally acceptable Item Sheet. A list of working definitions of cardinal terms was drawn up, duplicated and kept at hand by all taking part. These definitions related to certain symptoms, diagnoses and treatments but did not aim to be highly detailed, nor did they cover terms such as "duration of illness", "family history", etc. The intention was to avoid confusion due solely to verbal differences without attempting to obtain an artificial uniformity of concept: the former would have led to artefacts, while the latter were highly relevant to any realistic measure of reliability. The questions involved here are of some complexity and have been reviewed elsewhere (Kreitman, 1961). The Item Sheet itself contained an initial section concerning duration of illness, a check list of 28 major symptoms, details of

previous mental illnesses, the family history, diagnosis and treatment. (The details of the questions asked will become evident when reviewing the results of the investigation.) Each psychiatrist completed the Item Sheet immediately after seeing the patient.

The *patients* were unselected; they were simply 90 consecutive new referrals to the Chichester Service. One or two patients were known to the consultants prior to 1958 when the Service started, but since we began the investigation in late December, 1959, it is clear that these few cases had not been seen for at least 2 years. A general idea of the severity of the patients' illnesses can be obtained from Table I, while data on chronicity will emerge from the findings regarding duration of illness. Details regarding age, sex and social class are given in Tables IIa and IIb. The average age of the sample was 50.7 ± 2.05 years, S.D. = 19.31, and nearly half the sample were of social class III. Females predominated (71 per cent. of the total). They reflect fairly accurately the population characteristics of the patients referred to the Service in the past year, though young women tend to be over-represented at the expense of the middle-aged of both sexes.

TABLE IIa

Composition of Patient Sample by Age and Sex, Compared with All New Referrals in 1959

		Patient Sample. N=90			All New Referrals, 1959 N=865		
Age		Male %	Female %	Total %	Male %	Female %	Total %
Up to 40	..	10.0	33.3	43.3	16.0	19.0	35.0
41 to 60	..	7.8	16.7	24.5	11.5	22.5	34.0
Over 60	..	11.1	21.1	32.2	11.2	19.3	30.5
		28.9	71.1	100.0	38.7	60.8	99.5

TABLE IIb

Composition of Patient: Social Class

Class	..	..	I and II	III	IV and V	Unclassified	Total
No.	..	..	27	41	16	6	90

The six subgroups of 15 patients each did not differ significantly by age, sex or social class (see Appendix Tables 1, 2 and 3).

RESULTS*

It will be noticed that while in some instances it is meaningful to speak of numbers of patients, on other occasions it is only possible to refer to numbers of opinions, according to the context.

1. Duration of Illness

The Item Sheet required the doctor to state the duration of the patient's illness as he assessed it (a) on the basis of the patient's statements, and (b) on the basis of an informant's statement, whenever the latter was present. To assist analysis, six periods of time were listed for checking; they were: up to one week;

* Tables of negative findings are in the Appendix.

up to one month; up to six months; up to one year; up to two years; over two years.

The results, based on the psychiatrists' interpretation of the patient's statements only, are shown in Table III, which for simplicity excludes those patients who could not give an opinion about duration at both interviews. Adjustments have been made for apparent differences due solely to the interval between interviews. The diagonal in the Table shows precise agreement to have been reached in 43 cases, but in a further 9 patients exact agreement was reached by using the statement of informants. There were thus 52 (58 per cent.) agreed cases in all.

If the diagonal in the Table is widened by one point in both directions, so as to enlarge "agreement" to include approximate as well as exact concordance, then there are 69 patients (79 per cent.) on whom agreement was reached.

TABLE III

Agreement on Duration of Illness in 74 Cases Based on Patient's Statements

Second Opinion (X, Y)	First Opinion (A, B, C.)					
	Up to One Week	Up to One Month	Up to Six Months	Up to One Year	Up to Two Years	Over Two Years
Up to one week ..	5	1				
Up to one month ..		11	2		2	2
Up to six months ..	2	8	15		4	3
Up to one year ..			4	1		
Up to two years ..					1	
Over two years ..			4		2	10

Next, we sought the reasons, so far as our data permitted, for the inability of the psychiatrists to agree on duration of illness in 38 cases. Unexpectedly, it emerged that the presence of an informant had no effect on agreement (Appendix Table 4). Neither was there any overall association with different diagnostic groups (Appendix Tables 4, 5) though agreement was relatively lower when the patient was thought to have a psychoneurotic or personality disorder; when these two categories were compared with all the remainder, the difference reached the 5 per cent. level of significance (Table IV).

TABLE IV

Agreement on Duration of Illness and Certain Diagnostic Categories (Opinions)

				Duration		
				Agreed	Not Agreed	
Neurotic+Personality disorders		..	23	27	50	$\chi^2=3.9380$
Other diagnoses			81	49	130	df=1
				104	76	180
						p< .05

2. Family History

The section of the Item Sheet relating to family history required the examiner to indicate whether or not he thought the family to contain one or

more mentally ill members, and if so to specify whom, or whether he thought the patient or informant too unreliable to make a decision at all. It transpired that whenever a pair of psychiatrists agreed that there was a positive family history, in almost every case it was the same sick relative to whom they were referring. Hence it was possible to determine if the pair agreed on the basis of "positive", "negative" and "not known" categories only.

The results are shown in Table V, from which it will be seen that concordant opinions were obtained in 78 cases (87 per cent.). It should also be noted that in a further 6 cases disagreement was on the question of whether or not any assessment could be made at all, i.e. between "not known" or "negative".

TABLE V
Agreement on Family History

				First Opinion: A, B and C		
				Positive	Not Known	Negative
Second Opinion X and Y	Positive	..	..	25	0	3
	Not known	..	..	1	3	4
	Negative	..	..	3	1	50

In analysing the reason for disagreement, it was decided to omit, for simplicity and relevance, the three patients on whom the family history was agreed to be "not known". The opinions on the remaining 87 patients were not apparently influenced by examining the patient in the same "informant situation" (Appendix Table 6), nor did the diagnostic category have any bearing on the agreement attained (Appendix Table 7). Age, however, was significantly associated with disagreement (Table VI) and probably reflects increasing uncertainty on the part of the patient with respect to deceased relatives.

TABLE VI
Analysis of Opinion Regarding Family History by Age of Patient

				Family History		
				Agreed	Not Agreed	
Age	Up to 55	..	100	10	110	$\chi^2=5.5554$
	Over 55	..	50	14	64	df=1
				150	24	$p < .02$

3. Previous Illnesses

Examiners also recorded whether or not they thought the patient had been previously ill, or whether they considered it impossible to form an opinion. If they believed there had been previous illness, they were asked to specify the year or years in which this occurred, where treatment, if any, had been received, and whether the previous episodes resembled the current one. Only if both psychiatrists agreed on *all* these points was concordance of opinion taken as established.

The distribution of opinion is shown in Table VII, from which it will be seen that complete agreement was reached in 61 cases (68 per cent.).

TABLE VII
Distribution of Opinion Regarding Number of Previous Illnesses

				First Opinion: A, B and C				Not Known
				0	1	2	3 or More	
	0	..	..	42	10	1		2
Second	1	..	..	9	12+1*	2		
Opinion	2	..	..		3	5		
(X and Y)	3 or more	..					2	
	Not known	..	1					0

* Disagreement as to the characteristics of the previous attack.

It should also be noted that of the remaining 29 cases, 19 represent differences of opinion as to whether the patient had ever had a previous illness. Inspection of the notes reveals that in these instances the central difficulty was whether the current illness included a phase of partial as against total remission. In a further 3 cases, the disagreement arose over the trustworthiness of the data available.

On further analysis, agreement on previous illness was found to reflect neither the "informant situation" nor any *overall* association with diagnostic groups (Appendix Tables 8, 9). The latter Table does disclose, however, that higher than average agreement occurs in the organic group as compared with the remaining categories: this association is statistically significant (Table VIII).

TABLE VIII
Analysis of Opinion on Previous Illness by Certain Diagnoses

				Previous Illness		
				Agreed	Not Agreed	
Diagnostic Group:						
Organic	..	..	31	6	37	$\chi^2=5.4596$
Other groups	..	..	91	52	143	df=1
			122	58	180	$p<.02$

4. *Assessment of Symptoms*

In preliminary discussion a check list of 28 common symptoms with workable, if approximate, definitions, was drawn up. The purposes and difficulties in devising such lists will be reconsidered in the discussion of our findings. Each examiner was asked to mark for each patient which symptoms he considered important in deciding the diagnosis and disposal of the patient. This is not tantamount to deciding the presence or absence of a symptom, nor even grading its severity. Accordingly the results do not furnish an index of "reliability" in quite the same sense as used for the other aspect of examination already presented, since an assessment was required in addition to direct observation. The criterion of agreement was straightforward enough for most symptoms, being simply that it should be marked by both examiners. In some instances, however, such as with "somatic symptoms" or "disorder of thought" further specifications were requested, and agreement was scored only when these details also corresponded.

Percentages of agreement on the various symptoms were calculated as the proportion of all those patients in whom the symptoms had been found by either examiner, in which both examiners concurred, i.e.:

$$\frac{\text{No. of "agreed" patients} \times 100}{\text{No. of "agreed" + No. of "disagreed" patients}} = \% \text{ agreement}$$

The results are shown in Table IX, the left half of which gives the agreements obtained, while the right half shows the "incidence" of the symptoms.

TABLE IX
Symptoms: Agreement and Incidence

Symptom	Agreement of Symptom Assessment in 90 Patients				Incidence in 180 Opinions	
	No. Agreed	No. Dis- agreed	Agree- ment %	Rank	No.	Rank
Depression	59	10	85	1	128	1
Insomnia	44	25	64	2=	113	2
Over-activity	7	4	64	2=	18	15
Confusion, disorientation ..	12	7	63	4	31	9
Unable to care for self ..	8	6	57	5=	22	12
Disorders of consciousness	4	3	57	5=	11	20
Intellectual deterioration ..	9	7	56	7	25	11
Sexual problems	3	3	50	8	9	21=
Somatic	28	32	47	9	88	3
Loss of concentration ..	24	28	46	10	76	4
Hallucinations	4	5	44	11	13	17
Loss of memory	11	15	42	12	37	8
Anxiety	19	32	37	13	70	5
Phobias/Obsessions	3	6	33	14=	12	18=
Speech disorders	3	6	33	14=	12	18=
Suicidal	5	11	31	16	21	13
Delusions	6	16	27	17=	28	10
Other symptoms	4	11	27	17=	19	14
Retardation, anergy	9	25	26	19	43	6
Agitation	7	26	21	20	40	7
Depersonalization	1	4	20	21	6	24
Other disturbances of mood	0	15	0	22=	15	16
Aggression	0	9	0	22=	9	21=
Thought disorder	0	7	0	22=	7	23
Elation					} Below 6	
Delinquency						
Other behaviour disorders						
Autism						
	270	313.3	46.3		853	

From this Table it emerges that if depression, for example, is found by one psychiatrist and considered by him to constitute an important symptom in diagnosis and disposal, then in 85.5 per cent. of cases his colleagues will be of the same opinion. It should be noted that taking all symptoms together, the "reliability" or concordance is 46.3 per cent., a value which can also be interpreted as indicating the proportion of all symptoms which two psychiatrists are likely to agree upon when examining one "average patient".

Table IX poses the question of what determines the rank-order of agreement on symptoms, that is to say, makes for easy agreement on some symptoms and difficulty with others.

There is some suggestion that the symptoms at the top of the list are also those which are the most easily defined and which can be elicited with the minimum of inference, but on present evidence the question must remain open.

It appeared that there might be some association between the frequency with which a symptom is noted and its agreement value. A Spearman rank-order correlation coefficient was calculated and found to be $r_s = +.439$, which has a corresponding p value of beyond .05. The possible meaning of this association will be discussed later.

Further data on the relation between concordance on symptoms and other variables can be more conveniently presented in subsequent sections.

5. *Diagnosis*

The examiners agreed beforehand to use eleven diagnoses, only one of which could be marked for any one patient. Space was available for any special comment. The categories used will be seen in the Tables that follow. It was agreed that "reactive (neurotic) depression" should be used to designate patients whose major symptom was depression and who were not psychotic, though, as will be seen, it is doubtful if this definition was any more successful than the many others which have been proposed. In two diagnostic categories, namely, "personality disorder" and "other diagnoses" further specification was required, and an agreement was scored only when these details corresponded.

The distribution of opinion regarding diagnosis is shown in Table X. Some of the categories are too sparsely represented to warrant any comments, but it can be seen that the major areas of disagreement are between affective psychoses and reactive depression, and between the latter and anxiety states.

TABLE X
Distribution of Diagnostic Opinions at First and Second Examinations

Second Opinion (X and Y)				First Opinion (A, B and C)										Totals
				Affective Psychoses	Schizophrenia	Psychoses of Old Age	Other Organic Ills	Reactive (Neurotic): Depression	Anxiety States	Other Neuroses	Personality Disorder	Mental Defect	Nil Psychiatric	
Affective psychoses	..	..	31	2		1	3	1						38
Schizophrenia	..	..	1											1
Psychoses of old age	..	..			7	1								8
Other organic illness	..	..			1	8								9
Reactive (neurotic) depression		..	6	1		2	4	3					1	17
Anxiety states	..	..	1				1	3		1				6
Other neuroses	..	..					1	1	1					3
Personality disorder	..	..	1	1						1	1			4
Mental defect	..	..												0
Nil psychiatric	..	..	1									1		2
Other	..	..		1									1	2
Totals	..	..	41	5	8	12	9	8	1	2	1	1	2	90

It can also be seen that agreement of opinion, as shown on the diagonal in the Table, was reached in 57 patients (i.e. 114 opinions were concordant) or 63·3 per cent. of the total. This level of agreement will be designated "specific agreement". In a further 14* cases (representing 28 opinions), the two psychiatrists reached the same general diagnosis but disagreed at the specific level, as, for example, in the group of functional psychoses, where three cases occurred when one doctor diagnosed schizophrenia and the other affective psychosis. For these 14 patients (15·5 per cent. of the total) the term "generic agreement" will be used. Finally there were 19 cases (38 opinions, 21·1 per cent.) where no kind of agreement can be claimed on the diagnostic scheme used here. Further points arising in the interpretation of this Table will be raised in the discussion.

To discover what determined the various levels of agreement obtained, we analysed the three groups on all the variables mentioned so far. The results are shown in Tables XI to XV and Appendix Tables 10 to 13, in which, for clarity as well as logic, the analyses are based on the number of opinions in each group rather than on patients.

The presence or absence of an informant was not significantly associated with level of diagnostic agreement (see Appendix Table 10). It may be recalled that a similar lack of correlation has been shown for other variables in relation to availability of informants.

There was no association between agreement on the duration of illness and diagnostic concordance (Appendix Table 11) nor between the latter and agreement on family history (Appendix Table 12). It may further be noted (Appendix Table 13) that in those instances where both psychiatrists agreed on the family history, no apparent advantage accrues, so far as diagnostic concordance is concerned, if this is positive rather than negative.

On the other hand, agreement on the patients' previous illness was highly relevant to diagnostic concordance (Table XI). Moreover, the greater the number of previous illnesses the greater the likelihood of agreement on diagnosis (Table XII).

TABLE XI
Level of Diagnostic Agreement and Agreement on Previous Illnesses

			Previous Illness			
			Agreed	Not Agreed		
Level of Diagnostic Agreement	Specific	..	84	30	114	$\chi^2=9\cdot8374$
	Generic	..	12	16	28	df=2
	Nil	..	26	12	38	p<·01
			122	58	180	

TABLE XII
Level of Diagnostic Agreement and Number of Previous Illnesses

			Number of Previous Illnesses					
			Not Known	0	1	2 or More		
Level of Diagnostic Agreement	Specific	..	5	68	23	18	109	$\chi^2=13\cdot8917$
	Generic	..	1	14	11	2	27	df=4
	Nil	..	2	20	16	0	36	p< .01
			8	120	50	20	172	

* Two of these are represented in the "other diagnoses" column of Table X.

In Table XIII the levels of diagnostic agreement are compared with the percentage agreement on the major symptoms*. Our figures are unfortunately too small for detailed comparison except for the functional psychotics and the neurotics. In the former group there is a slight trend in the expected direction, that is to say, there is higher agreement on symptomatology in those cases where specific diagnostic agreement is attained, and progressively lower levels in the "generic" and "nil" categories, but this trend does not appear with the neurotic cases. For the total group, moreover, the trend is not significant.

TABLE XIII

Level of Diagnostic Agreement and Percentage Agreement on Major Symptoms

			Diagnostic Group				
			Organic	Functional Psychosis	Neurosis	Other	All
Level of	Specific	..	47.9	48.6	47.4	50.0	48.3
Diagnostic	Generic	..	32.2	45.4	46.5	—	43.5
Agreement	Nil	..	44.4	42.9	51.2	13.3	42.1

Italicized values are based on less than 10 patients. The percentages shown in the "All" column do not differ at an acceptable level of confidence.

Finally, it may be recalled (Table X) that some diagnostic categories show higher concordance of opinion than others. The data has been recast† in Table XIV, from which it may be seen that if one doctor diagnoses, for example, psychosis of old age, his colleague is much more likely to agree with him than if he has made, say, a diagnosis of anxiety neurosis.

TABLE XIV

Level of Diagnostic Agreement by Diagnostic Category (Opinions)

		Diagnostic Group				
Level of Diagnostic Agreement		Organic	Functional Psychosis	Neurosis	Personality Disorder and Other	
Specific	..	30	62	16	6	114
Generic	..	4	10	14	0	28
Nil	..	3	15	14	6	38
		37	87	44	12	180

$\chi^2=25.3653$
df=6
p<.001

For purposes of comparison with other studies, we have calculated (Table XV) in terms of *patients* the percentage for whom the specific and generic level of diagnoses are obtained in each diagnostic category‡.

TABLE XV

Level of Diagnostic Agreement by Diagnostic Category (Patients)

Level of Diagnostic Agreement		Organic	Functional Psychoses	Neuroses	All
Specific	..	75%	61%	28%	63%
Specific plus Generic	..	85%	71%	52%	78%

* Symptoms of major importance for the differential diagnosis and treatment of the patient.

† The transposition has involved detailed consideration of each pair of opinions, so that it will not always be easy for the reader to deduce the figures shown in the "generic" and "nil" groups from the distribution given in Table X.

‡ For this purpose the nil agreement values shown in Table XIV must be doubled in each category, since the second non-concordant opinion in the same row will be in another column. In this transposition each patient is, of course, represented by two opinions.

6. Treatment

The Item Sheet contained a list of ten main forms of therapy. The examiners were requested to indicate which of these they would adopt as the initial line of therapy, and space was also provided to mark possible second and third procedures in the therapeutic plan. Agreement at a specific level was attained if (a) both examiners indicated the same therapy for the patient, giving only one procedure each, and (b) if the first line of treatment recommended by either one examiner corresponded with the first or second recommendation of the other. The second criterion was adopted since in many instances the sequence of treatments was somewhat arbitrary and it appeared unwarranted to designate such differences as important disagreements.

It was found that in 57 cases (63.3 per cent.) specific agreements were attained*, while in a further 4 (4.4 per cent.) the disagreements consist only of whether the patient should receive "consultation and advice" from the doctor or the P.S.W., or whether he should have "supportive psychotherapy". Disagreements of this minimal nature may well be embraced in a category of "general agreement". In the remaining 29 cases (32.2 per cent.) no agreement was attained on the basis of the categories used here, though sometimes disagreement was slender; as for example, between "consultation and advice" and "pharmacological with supportive therapy", where a decision to prescribe sleeping tablets as part of a general supportive programme resulted in a major shift of category. The problems of imposing categories seemed greater in this section than in any of the preceding.

We next examined how agreement on treatment was associated with other variables. It was found (Table XVI) that there was a slightly higher concordance regarding symptoms with those patients for whom therapy was agreed, but the difference was not significant.

TABLE XVI
Agreement on Treatment and on Symptoms

	No. of Patients	No. of Symptoms		% Agreement
		Agreed	Not Agreed	
Treatment specifically agreed ..	57	182	196	48.1
Treatment not specifically agreed*	33	88	117	42.9

* The "general agreement" and "no agreement" treatment categories have been taken together as the numbers are not large enough to warrant more detailed comparison.

Similarly, it was discovered (Table XVII) that no significant association

TABLE XVII
Agreement on Treatment and on Diagnosis

Level of Diagnostic Agreement (Opinions)				Treatment		
				Agreed	Not Agreed	
Specific	..	..	..	74	40	114
Generic	..	..	..	18	10	28
Nil	..	..	..	22	16	38
				114	66	180

* Since multiple choice was available, a table of the distribution of opinion would be too complex to be readily interpreted.

existed between concurrence on treatment and the *overall* level of diagnostic agreement but an association did emerge for the patients thought to be functional psychotics ($p < .02$).

These findings might suggest at first glance that neither symptoms nor diagnosis have any implications for therapy. We were able to demonstrate, however, that there exists a very highly significant association between the generic diagnosis made by the psychiatrist and the forms of treatment he subsequently recommends (Table XVIII).

TABLE XVIII
Choice of Therapy by Diagnostic Opinion

			Organic	Functional	Neurotics		
			(N=37)	Psychoses (N=87)	(N=44)		
Psychotherapy and support	..	..	8	7	16	31	$\chi^2=107.5392$
Pharmacological	..	..	13	51	30	94	df= 6
E.C.T.	..	..	0	49	3	52	$p < .001$
Other, or nil	..	..	23	2	6	31	
			44	109	55	208	

As might have been expected, there were significant differences in the proportions of "treatment agreed" patients among the different diagnostic categories, being maximal in those diagnosed as functional psychoses (Table XIX).

TABLE XIX
Agreement on Treatment by Diagnostic Opinion

Diagnostic Opinion		N.	Treatment (All Forms)		
			Agreed %	Not Agreed %	
Functional psychosis	..	87	77	23	$\chi^2=15.41$
Neurosis	..	44	54	46	df= 3
Organic disease	..	37	51	49	$p < .01$
Other	..	12	33	66	

We were unable to determine why agreement on treatment fails (for the group as a whole) to reflect the level of agreement on diagnosis. The "informant situation" and the social class of the patient were without any demonstrable influence. We must, therefore, assume that other variables not covered by our data are responsible.

Lastly, we found that the highest concordance on treatment occurs with E.C.T. (77 per cent. agreement), and with psychotherapy and supportive therapy (76 per cent.), followed by pharmacological therapy (42 per cent.). The low value for the latter may to some extent be due to "category error". The differences between these agreement levels were highly significant statistically ($p < .001$).

7. Differences Between Psychiatrists

We wished to discover whether there were any important individual differences between the ability of the various investigators to reach agreement

with their colleagues, and if so, with respect to which variables. It will be recalled that each group of 15 patients examined by each pair of doctors has been shown to be of equivalent age, sex and social class, though there was some discrepancy in the average interval elapsing between examinations.

There were many possible ways of assessing individual performance, all of which led to essentially the same result. The method used was to sum the numbers of patients on which each doctor had reached specific agreement with his colleagues and to construct a simple contingency table based on "agreed" and "not agreed" patients for each investigator. The data regarding their agreements on duration of illness, family history, previous illness, symptomatology, diagnosis and treatment are set out in Appendix Tables 14 to 19. It will be seen that for none of these does any significant discrepancy between psychiatrists emerge.

DISCUSSION

Estimation of the reliability of clinical assessment is beset with numerous difficulties of both a theoretical and practical nature. These have been discussed elsewhere (Kreitman, 1961). Our present findings, however, warrant some further comment.

Firstly, the various criteria of concordance used should be considered. Had different ones been stipulated, our results would doubtless have been different. Nor would it have been difficult to attain higher reliabilities by using operational definitions more extensively; to have done so while maintaining that it was the ordinary clinical situation which was being examined would have been inapposite. The clinician cannot use many of the operational criteria which can be legitimately be employed for specialized purposes. He cannot define, for example, a "previous illness" as a "previous hospital attendance", since many patients give histories of previous episodes of untreated ill-health. Hence what may to the epidemiologist be an acceptable definition, and probably one of high inter-observer reliability, is of no value to the service psychiatrist. In our choice of definitions and in our criteria of agreement we have been obliged to be faithful to the actual situation which pertains in our type of hospital and with our type of patient. This means that to some extent our criteria are arbitrary, but they seem reasonable.

Secondly, we must point out that results obtained in studies of this nature are likely to be underestimates of the true situation; the imposition of categories on continuous variables—such as many of the phenomena considered by psychiatrists are generally acknowledged to be—must involve distortion, as must the arbitrary separation of overlapping groupings. This problem we encountered most acutely in our analysis of recommendations for therapy, but it is doubtless of widest importance when considering diagnosis, where a slight difference of emphasis between two opinions may result in their being classified as discordant. We hope to have gone some way towards overcoming this problem by using whenever possible both "specific" and "generic" level of concordance, but it would be unwarranted to assume this has been an adequate answer in every instance.

We may now turn to the results. Agreement on duration of illness utilizing both the patients' and the informants' statements, was reached in about 60 per cent. of cases. We found that for the group as a whole the presence of an informant did not significantly affect agreement levels. The apparent failure of the informants to assist may have been because they were disproportionately fewer in the cases where they were most needed, namely, with those patients

diagnosed as neurotics. In this group the same informant was available for both psychiatrists in only 16 per cent. of the interviews, compared with 32 per cent. for all the other patients. The relatively greater difficulty in assessing duration of illness in neurotic patients (Appendix Table 5) probably reflects the insidious onset of the illness in many of them.

Agreement on family history was approximately 85 per cent. The only significant association with agreement on family history was the patient's age; the older the patient the less the pair of psychiatrists were able to agree. As previously suggested this may well be due to increasing uncertainty on the patient's part about long-deceased relatives, in addition to other considerations such as failing memory.

The criteria for agreement on previous illness were perhaps the most stringent of all, yet approximately 70 per cent. agreement was reached. This finding is of some importance in view of the association of agreement on previous illness with agreement on diagnosis, which will shortly be discussed. The only other variable found to be associated with agreement on previous illness, namely the diagnostic category in which the patient was placed, appears to be true only for the organic-non-organic dichotomy. In the former group a relatively higher proportion of opinions were concordant on previous illness (albeit it was usually agreed that there was none), but it may be that enquiries were not pushed very far with these patients as the diagnosis was often obvious at a glance.

In discussing the agreement attained in the assessment of symptoms it is important to repeat that we were not concerned solely with the reliability with which any given symptom could be rated, and the qualifications which precede the presentation of our findings should be borne in mind. Our aim in designing this section was to compare how different examiners assessed the patients' symptomatology with reference to differential diagnosis and treatment. The range of concordance values obtained was from 0 per cent.-85 per cent., with the highest value, relating to "depression", markedly ahead of all the others. This can be taken to mean that as a symptom depression is both generally regarded as being important in deciding the diagnosis and sufficiently clear-cut for agreement to be reached in over eight out of ten cases where it is considered by either examiner to be present. It similarly follows that our "incidence" figures must be interpreted not as absolute values relating to the presence of various symptoms, but of their presence *when important*.

The positive rank-order correlation between agreement and incidence is not a finding that could have been predicted with confidence, since one might equally well have anticipated that certain uncommon but distinctive symptoms would attain the highest agreement levels. There appear to be two possible explanations. Firstly it may be that common symptoms are those in the assessment of which psychiatrists are most expert and hence those on which they achieve greater inter-observer agreement. A second possibility is that the correlation obtained is to some degree an artefact. If two psychiatrists were in the habit, with or without justification, of frequently detecting some symptoms and rarely finding others, then they may on the basis of chance alone reach higher "concordance" on the former group. Reflection suggests, however, that simple probability theory cannot legitimately be applied in the present context to correct for such chance factors. Which of these two explanations is correct thus remains an open question.

The findings regarding agreement on diagnosis are of particular interest. It will be recalled that agreement when using broad diagnostic categories was

attained in about 80 per cent. of cases, and when specific diagnosis were used in about 65 per cent. In about half the cases where total disagreement occurred, one psychiatrist had diagnosed reactive or neurotic depression, the other affective psychoses. This particular dichotomy of opinion seems in our sample to represent the greatest single source of diagnostic confusion. General experience would suggest that this might also be true at other centres. Yet it must also be noted that although this particular disagreement arose in nine cases, in the further 35 it was possible for the two psychiatrists to agree, using the same categories. With one important exception, efforts to demonstrate how agreement on various subsidiary issues correlated with the level of diagnostic agreement were all without success; the availability of informants and whether they were the same or differed on two occasions; the duration of illness; the family history, and whether this was positive or negative; what assessment was made of the major symptoms—agreement on none of these showed acceptable levels of association with the level of diagnostic agreement. Agreement on the previous history of psychiatric illness, however, yielded a positive correlation and here the association was striking. Moreover where the patient's previous illnesses were known, the level of diagnostic agreement attained increased in proportion to the number of such illnesses. This association (between level of diagnostic agreement and agreement on previous illness) is not a function of an unduly high reliability in the latter, which was about 70 per cent., while that found for family history, for example, was over 85 per cent.

Yet clearly there must be other determinants of diagnostic agreement since in a number of cases agreement on diagnosis was reached despite disagreement on previous history. It is likely that although no differences can be shown in agreement on symptom assessment as a whole between various levels of diagnostic concordance, nevertheless certain individual symptoms have a near-pathognomonic quality for a specific diagnosis: intellectual deterioration is a case in point.

A further possibility arises from consideration of the proportions within the diagnostic categories attaining the various levels of diagnostic concordance (Table XIV and XV). Agreement at the "specific" level was reached in 81 per cent. of instances when either psychiatrist diagnosed an organic disorder, in 72 per cent. of those recorded as functional psychoses, but in only about 36 per cent. of those diagnosed as neurotic. Between the last two groups it has been shown (Appendix Table 9) that there is no difference between the agreement attained on previous psychiatric history, and other variables have been shown to have no immediate relevance. It follows that differences in diagnostic agreement must be due to quite another factor altogether, and it is suggested that this may be the *concepts* which the psychiatrists entertain of the various illnesses embraced in these categories. Despite many remaining problems it is probably true that we are clearer about the varieties of functional psychoses than on the various forms of neurosis.

The role of concept of illness in diagnostic reliability is a difficult one to assess and we would not wish to urge that our data does more than support the general idea of its relevance. Measurement of its effects will have to await further research especially designed to that end, as for example by an investigation using standardized material.

Comment on the findings regarding treatment is difficult because the problems of classification were so formidable. It is gratifying to note, however, that diagnosis was shown to have clear implications for therapy and that the agreement attained for E.C.T. (approximately 80 per cent.) and for psycho-

therapy in its various forms (approximately 75 per cent.), are reasonably high. The much lower value for pharmacotherapy (approximately 40 per cent.) presumably reflects uncertainty on the use of the many new drugs. It has also been shown that, within the group of functional psychoses, agreement on therapy reflects agreement on specific diagnosis. What determines agreement or disagreement on treatment in the other cases remains obscure.

Individual differences between psychiatrists were found to be of little importance in this study; each participating doctor attained approximately equal agreement levels with his colleagues on all the variables considered. It should be noted, however, that the pairing was always between one of the full-time research psychiatrists and one of the service consultants. We cannot tell if either group would have attained higher or lower internal consistency than the between-group values reported.

Finally it may be observed that we were unable to demonstrate the usefulness of our informants. It was not possible to show on any of the variables examined that higher agreement was obtained for the group as a whole when informants were available than when they were not. This may be because our data does not extend to assessment of personality, domestic background, etc., and also because our sample included so few patients who were severely disturbed or unco-operative.

There have been only four other studies utilizing the paired-opinion technique in diagnostic reliability studies. These have been reviewed in detail elsewhere, but the main findings have been summarized in Table XVIII. The data on the percentages of specific agreement are less informative than those for the generic level, since it is not always stated how many alternative diagnoses have been available to the examiners, and whether the schedule employed was fully represented in the patient sample. This applies less cogently to the broader categories, but where data are reported for other groupings, such as "neurosis and psychopathy", a gap has been left in the Table. Two conclusions can be drawn from this data. Firstly, higher reliability has always been attained in organic cases than in the functional psychoses, and higher values reached in the latter than with neurotics. Secondly, the levels attained by the various investigations within each group appear to reflect the severity of illness in their sample. This trend is consistent, and would appear to be of major importance, but many further studies will be required before such a conclusion can be anything but tentative.

Other investigations have not been concerned with an analysis of how diagnostic agreement is attained nor with what other clinical assessments such agreement is correlated. Our own findings strongly suggest that it is above all agreement on the previous psychiatric illness which contributes most to agreement on diagnosis.

SUMMARY

1. Six groups of patients, each of 15 new referrals to the Chichester Mental Health Service, were independently examined by pairs of psychiatrists at approximately three-day intervals. The groups were homogeneous with respect to age, sex and social class.

2. Agreement on duration of present illness was attained in about 60 per cent. of patients, being lowest for the patients diagnosed as neurotic.

3. Agreement on family history was obtained in about 85 per cent. of patients, being lowest with those over 55 years of age.

4. Agreement on previous psychiatric illness was reached in about 70 per cent. of patients with higher agreement among those diagnosed as having an organic illness.

5. On comparing the assessments made on symptomatology, agreement values of between 85 per cent. (for depression) and 0 per cent. (for aggression, thought-disorder) were found. The final rank order correlated at a significant level with that for the incidence of the various symptoms.

6. Agreement on diagnosis, using an agreed list of 11 possible diagnoses, was attained at a specific level in about 65 per cent. of the patients and at a broader or generic level in a further 15 per cent. Agreement on diagnosis was strongly associated with agreement on previous history and with the number of previous illnesses. On the other hand there was no demonstrable association with agreement on symptoms or with the other variables mentioned. Highly significant discrepancies emerged between the agreement levels achieved in different diagnostic categories, being highest with organic patients and lowest with neurotics.

7. Agreement on therapy (based on a choice of 10 treatments) was found in 65 per cent. of the cases, with approximate agreement in a further 5 per cent. The level was considerably higher in the functional psychoses than in the remaining patients and in the former group it was also shown that agreement on therapy reflected diagnostic agreement, though this association could not be substantiated for the total sample. It was also shown that diagnosis has significant inferences for therapy. E.C.T. and psychotherapy were recommended with a far higher level of concordance (each about 75 per cent.) than was pharmacological therapy (40 per cent.).

8. No important differences could be demonstrated between the ability of the five psychiatrists taking part in reaching agreement with their colleagues on any of the variables enumerated above.

9. Comparison of our findings with those of other workers confirms that concordance of diagnostic opinion varies according to the diagnostic group and also suggests that higher levels are attained with more severely disturbed patients.

TABLE XX
Summary of Previous Investigations

Authors	Organic Disorders		Functional Psychoses		Neuroses		All Diagnoses		Severity of Illness
	Gen- eric %	Speci- fic %	Gen- eric %	Speci- fic %	Gen- eric %	Speci- fic %	Gen- eric %	Speci- fic %	
Ash (1949)	—	—	—	—	—	—	58-68	33-43	Mild: O.P. screening centre
Hunt, Wittson and Hunt (1953)	—	—	59	31	24	12	54	33	Moderate: I.P. screening centre
Present study	85	75	71	61	52	28	78	63	Average range: O.P. and I.P. hospital referrals
Schmidt and Fonda (1956)	92	74	80	47	—	—	84	55	Average range: I.P. hospital ad- missions
Norris (1959)	—	—	89	66	—	—	—	—	Severe: observa- tion wards

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APPENDIX TABLE 1

Analysis of Variance of Age of Patients in Paired-Psychiatrist Groups
N=15 Each Group

Source	d.f.	Total Variance	Mean S.S.	Variance Ratio	
Between groups ..	5	1,986.890	397.3780	1.0704	M = 50.7555
Within groups ..	84	31,183.733	371.2349		S.D. = 19.3055
					S.E. = 2.05

F ratio not significant at the 5 per cent. level.

APPENDIX TABLE 2

Distribution of Patients by Sex Among Six Pairs of Psychiatrists

	Male				Female				
A.X.	..	..	..	3	12	15	$\chi^2=3.044$	d.f.=5	p < .70
B.X.	..	..	..	6	9	15			
C.X.	..	..	..	4	11	15			
A.Y.	..	..	..	3	12	15			
B.Y.	..	..	..	4	11	15			
C.Y.	..	..	..	6	9	15			
Total	..	..		26	64	90			

APPENDIX TABLE 3

Distribution of Patients by Social Class Among Six Pairs of Psychiatrists

(Excluding 6 unclassified patients)

	I and II				III	IV and V		
A.X.	..	..	..	5	5	4	14	$\chi^2=7.872$
B.X.	..	..	..	4	7	3	14	
C.X.	..	..	..	4	6	4	14	
A.Y.	..	..	..	7	6	1	14	d.f.=10
B.Y.	..	..	..	3	7	3	13	
C.Y.	..	..	..	4	10	1	15	
				27	41	16	84	p < .70

APPENDIX TABLE 4

Analysis of Opinion Regarding Duration: Agreement by "Informant Situation"

					Duration		
					Agreed	Disagreed	
Informants:	Same	..	..	..	28	22	50
	Anomalous	..	..	..	18	11	29
	Nil	..	..	..	58	43	101
					104	76	180

 $\chi^2 = 0.2889$
d.f. = 2
 $p < .09$
(
 χ^2 does not reach significant level if "anomalous" cases are compared with the remainder.)

APPENDIX TABLE 5

Analysis of Opinion Regarding Duration: Agreement by Diagnostic Groups (Opinions)

					Duration		
					Agreed	Not Agreed	
Organic	..	..	..	..	25	12	37
Functional psychosis	..	..	..	..	51	36	87
Neurotic	..	..	..	..	20	24	44
Personality disorder	..	..	..	..	3	3	6
Other	..	..	..	..	5	1	6
					104	76	180

 $\chi^2 = 6.0742$
d.f. = 4
 $p < .20$

APPENDIX TABLE 6

Analysis of Opinion Regarding Family History
(Omitting 3 cases agreed to be "not known")

					Family History		
					Agreed	Disagreed	
Same	..	..	..	..	39	6	45
Anomalous	..	..	..	..	23	5	28
More	..	..	..	..	88	13	101
					150	24	174

(
 χ^2 does not reach a significant level if "Anomalous" cases are compared with the remainder.)

APPENDIX TABLE 7

Analysis of Opinions Regarding Family History by Diagnostic Category
(Omitting 3 cases agreed to be "not known")

					Family History		
					Agreed	Disagreed	
Organic	..	..	..	..	25	6	31
Functional psychosis	..	..	..	..	78	9	87
Neurotic	..	..	..	..	39	5	44
Personality and other disorders	..	..	..	..	8	4	12
					150	24	174

 $\chi^2 = 2.752$
d.f. = 3
 $p < .50$

APPENDIX TABLE 8

Analysis of Opinion Regarding Previous Illnesses by Informant Situation

					Previous Illnesses		
Informant Situation					Agreed	Disagreed	
Same	..	..	..	..	32	18	50
Anomalous	..	..	..	..	19	10	29
None	..	..	..	..	71	30	101
					122	58	180

 $\chi^2 = 0.6886$
d.f. = 2
 $p < .80$
(
 χ^2 does not reach a significant level if "Anomalous" is compared with the remainder.)

APPENDIX TABLE 9

Analysis of Opinions Regarding Previous Illness by Diagnostic Category

Diagnostic Group					Previous Illnesses		
					Agreed	Disagreed	
Organic	..	..	..	..	31	6	37
Functional psychosis	..	..	..	..	55	32	87
Neurotic	..	..	..	..	29	15	44
Personality disorder	..	..	..	..	3	3	6
Other	..	..	..	..	4	2	6
					122	58	180

$\chi^2 = 6.9860$
d.f. = 4
p < .20

APPENDIX TABLE 10

Level of Diagnostic Agreement and Informant Situation

Diagnostic Agreement					Informant Situation			
					Present	Anomalous	Nil	
Specific	..	..	..	..	36	18	60	114
Generic	..	..	..	..	6	6	16	28
Nil	..	..	..	..	8	5	25	38
					50	29	101	180

(χ^2 does not reach an acceptable level of significance if "Anomalous" is compared with the remainder.)

APPENDIX TABLE 11

Level of Diagnostic Agreement and Agreement on Duration of Illness

Diagnostic Agreement					Duration		
					Agreed	Not Agreed	
Specific	..	..	..	..	70	44	114
Generic	..	..	..	..	18	10	28
Nil	..	..	..	..	16	22	38
					104	76	180

$\chi^2 = 4.9187$
d.f. = 2
p < .10

APPENDIX TABLE 12

Level of Diagnostic Agreement and Agreement on Family History

(Excluding opinions on 3 patients whose family histories were agreed to be "not known")

Diagnostic Agreement					Family History		
					Agreed	Not Agreed	
Specific	..	..	..	..	96	12	108
Generic	..	..	..	..	22	6	28
None	..	..	..	..	32	6	38
					150	24	174

$\chi^2 = 2.0298$
d.f. = 2
p < .50

APPENDIX TABLE 13

Level of Diagnostic Agreement and Nature of (Agreed) Family History

Level of Diagnostic Agreement					Family History Agreed to be:		
					Positive	Negative	
Specific	..	..	..	..	32	69	96
Generic	..	..	..	..	10	12	22
Nil	..	..	..	..	8	24	32
					50	100	150

$\chi^2 = 2.5198$
d.f. = 2
p < .30

(The three cases (6 opinions) agreed to be "not known" were all specific agreements diagnostically.)

APPENDIX TABLE 14

Numbers of Patients Agreed with Respect to Duration, by Psychiatrists

Psychiatrist						Agreed	Not Agreed		
A	..	..	..	..	..	19	11	30	$\chi^2 = 2.2772$
B	..	..	..	..	..	16	14	30	
C	..	..	..	..	..	17	13	30	d.f.=4
X	..	..	..	..	..	23	22	45	
Y	..	..	..	..	..	29	16	45	$p > .70$
						104	76	180	

APPENDIX TABLE 15

Number of Patients Agreed with Respect to Family History, by Psychiatrists

Psychiatrist						Agreed	Not Agreed		
A	..	..	..	..	..	28	2	30	$\chi^2 = 5.7692$
B	..	..	..	..	..	24	6	30	
C	..	..	..	..	..	26	4	30	d.f.=4
X	..	..	..	..	..	36	9	45	
Y	..	..	..	..	..	42	3	45	$p > .30$
						156	24	180	

APPENDIX TABLE 16

Number of Patients Agreed with Respect to Previous Illness, by Psychiatrists

Psychiatrist						Agreed	Not Agreed		
A	..	..	..	..	..	19	11	30	$\chi^2 = 3.2044$
B	..	..	..	..	..	24	6	30	
C	..	..	..	..	..	24	6	30	d.f.=4
X	..	..	..	..	..	31	14	45	
Y	..	..	..	..	..	30	15	45	$p > .70$
						122	58	180	

APPENDIX TABLE 17

Number of "Agreed" and "Not Agreed" Symptoms by Psychiatrists

						Symptoms			
Psychiatrist						Agreed	Not Agreed		
A	..	..	..	..	..	96	99	195	$\chi^2 = 2.9752$
B	..	..	..	..	..	99	116	215	
C	..	..	..	..	..	75	98	173	d.f.=4
X	..	..	..	..	..	120	156	276	
Y	..	..	..	..	..	150	157	307	$p < .70$
						540	626	1,166	

APPENDIX TABLE 18

Levels of Diagnostic Agreement by Psychiatrists

						Level of Diagnostic Agreement			
						Specific	Generic	Nil	
A	..	..	..	..	..	18	3	9	$\chi^2 = 11.8528$
B	..	..	..	..	..	22	5	3	
C	..	..	..	..	..	17	6	7	d.f.=8
X	..	..	..	..	..	34	3	8	
Y	..	..	..	..	..	23	11	11	$p < .20$
						114	28	38	

APPENDIX TABLE 19

Number of Patients Agreed with Respect to Treatment, by Psychiatrists

						Agreed	Not Agreed		
A	..	..	..	..	..	18	12	30	$\chi^2 = 1.2917$
B	..	..	..	..	..	18	12	30	
C	..	..	..	..	..	21	9	30	d.f. = 9
X	..	..	..	..	..	27	18	45	
Y	..	..	..	..	..	30	15	45	p < .90
						114	66	180	

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THE INCIDENCE OF TEMPORAL LOBE EPILEPSY AMONG A HOSPITAL POPULATION OF LONG-STAY FEMALE EPILEPTICS*

By

J. H. MARGERISON

and

D. W. LIDDELL

INTRODUCTION

THERE are some 23,000 long-stay epileptic in-patients in Britain. Most of these patients are in hospitals for the mentally ill or subnormal or in independent colonies. There are, however, only two hospitals within the National Health Service which cater specifically for the long-term treatment of epilepsy, St. Faith's Hospital, Brentwood, for females, and St. David's, Edmonton, for males. This communication is concerned with the incidence of temporal lobe epilepsy among the long-stay female epileptics at St. Faith's.

Belinson (1947) reported that 26 per cent. of a group of epileptic in-patients showed psychomotor type seizure discharges in their EEGs. However, most of these patients had been admitted with the primary diagnosis of mental deficiency and EEG information obtained from single 20-minute standard recordings. Roger and Dongier (1950) analysed the clinical and EEG features in 50 mental hospital epileptics and found EEG evidence of involvement of the temporo-hippocampo-perifalciforme area in 84 per cent. of the 38 cases in which admission was because of outbursts of impulsive and/or aggressive behaviour. Liddell (1953) reported a temporal focus in 50 per cent. of the epileptic patients in a mental hospital. There does not appear to have been any previous survey of the incidence of temporal lobe epilepsy among those long-stay epileptics who are supposed to have been hospitalized primarily on account of their epilepsies.

Jackson's (1899) term "uncinate group of seizures" was based on a clinico-pathological association in that seizures involving "dreamy states" were found to occur in patients in whom autopsy studies subsequently revealed gross lesions of the temporal lobe. He was careful to point out, however, that "the discharge lesions in these cases are made up of some cells, not of the uncinate group alone, but of some cells of different parts of a region of which this gyrus is part—a very vague circumscription, I admit—the uncinate region."

In recent times and with the advent of EEG, circumscriptions have become more rather than less vague, so that comparison between different studies of temporal lobe and/or psychomotor epilepsy is rarely possible. Lennox (1960), in a chapter entitled Psychomotor (Temporal: Psychic) Epilepsy, did not offer any operational definition nor did one emerge from the Second International Colloquium on Temporal Lobe Epilepsy (1957). Bingley (1958) referred to this

* This work was carried out at Runwell Hospital, Wickford, Essex, with monies provided by the Dowager Lady Peel Trust and under the auspices of the Mental Health Research Fund from whom one of us (J.H.M.) held a research fellowship.

problem and himself elected to classify on two criteria, namely, the presence of epileptic seizures and the presence of a temporal EEG focus and/or a verified temporal lobe glioma. As Northfield (1958) pointed out, "there is much evidence that forms of epilepsy which we associate with temporal lobe disorder can arise from lesions outside the temporal lobe, but little evidence that these highly complicated seizures are exclusively disturbances of temporal lobe function." Fulton's (1953) suggestion that the group of seizures in question "might more properly be designated as seizures involving the limbic lobe" is an attractive way out of the dilemma but to date has not proved testable in man.

Estimates of the incidence of temporal lobe epilepsy involve the primary assumption that such a condition exists. If there is reasonable doubt about the specificity of diagnostic criteria then the survey should be carried out in such a way that others can repeat the work and results be compared. For the purposes of the present study concordant EEG and clinical data, separately assessed and without cross contamination between the two sources of evidence, was the operational basis upon which a diagnosis of temporal lobe epilepsy was to have been made.

METHOD

The entire population of 270 females was surveyed in the following way:

(a) *Clinical Information*

1. Existing hospital records of each patient were summarized.
2. Interviews were carried out by one of us (J.H.M.). In most cases they lasted for several hours and involved first obtaining a free history and thereafter asking questions deliberately slanted with a view to completing a questionnaire. The questionnaire had been designed to cover as widely as possible the symptomatology of epilepsy in general and of the clinical concept of temporal lobe epilepsy in particular.
3. A fit report was made in respect of each patient by a nurse familiar with the usual seizure pattern.
4. Neurological examinations were carried out by one of us (D.W.L.) on all patients. No other evidence, clinical and/or EEG, was made available to the examining clinician.

(b) *EEG Information*

EEGs were taken with an eight-channel Edison Swan machine. The electrode positions used were the same as those adopted by Hill at the Maudsley Hospital, London, and have been described by Pampiglione (1956). An important difference from the 10-20 system is the use of an additional sylvian electrode. The position of the electrodes with respect to underlying cerebral cortex was determined on autopsy specimens at the start of the survey (Darby, Lettich and Margerison, 1958).

In nearly all cases a minimum of three EEGs were carried out, a resting record with hyperventilation (3 minutes) and photic stimulation, a quinalbarbitone induced sleep tracing and a sphenoidal EEG under thiopentone. The technique for sphenoidal recordings followed that described by Pampiglione and Kerridge (1956) being based on earlier work by Jones (1951) and by Kerridge (1952). An additional refinement was the use of wire electrodes threaded through the sphenoidal needle and left *in situ* after withdrawal of the needle, following again the routine practice in the EEG Department of the

Maudsley Hospital. Children (patients below the age of 16) and the elderly infirm were in general excluded from the sphenoidal investigation and where the EEG data was incomplete patients were put into a "not enough evidence" category for the purpose of assessment of incidence figures.

EEGs were interpreted by one of us (J.H.M.) without reference to other information and this was achieved by adopting a number coding for the patients on the survey. The criterion used for EEG evidence in favour of temporal lobe epilepsy was the occurrence of spikes or sharp waves in the anterior and/or middle and posterior temporal regions. In order to check the value of spike counting as a procedure an inter-rater reliability correlation study was subsequently carried out by three members of the EEG Department of Runwell Hospital and correlation coefficients of 0.97 or better established between these raters in 66 counts.

In view of the observations of Kennedy and Hill (1958) on the prognostic significance of localized diminution in barbiturate-induced fast rhythms between the sphenoidal and ear electrodes on the side of the focus in cases of temporal lobe epilepsy, this was made an additional factor in EEG analysis and will be considered separately.

(c) *Assessment of the Information*

All the clinical information was collected and summarized. It was then read out in summary form to us and we attempted independently to classify the patients with respect to the presence or absence of temporal lobe epilepsy on clinical grounds. It is not possible to lay down in precise terms the criteria for clinical diagnosis because while most clinicians specializing in the subject are in general agreement as to the reality of the symptom complex there is no such agreement as to what may be included or must be excluded. Furthermore, it is not yet known what differential weighting should be applied to the multiplicity of symptoms which may be encountered. Gastaut's (1953) long list of symptoms of psychomotor epilepsy, "perhaps too inclusive" according to Lennox (1960), was for instance known to both of us. We were also familiar with Penfield and Jasper's (1954) classification. In general, the basis for clinical diagnosis was that described by Hill (1953). "Objective or subjective vertigo", however, when described as an aura was not considered of itself to point towards a diagnosis of temporal lobe epilepsy. If vertigo, however, was preceded, accompanied or followed by some other temporal lobe symptom as, for instance, in combination with an epigastric sensation and/or a feeling tone of fear, then it was considered to point to such a diagnosis.

After the clinical and EEG information had been separately collected and analysed, correlations were sought between the clinical diagnoses of the two clinicians and between EEG and clinical data. Incidence figures based on these assessments were then calculated.

Arrangements were made at the start of the survey in 1956 for autopsy material to be collected and neuropathological evidence is gradually being accumulated by Dr. J. A. N. Corsellis. Reference will be made to the provisional findings and a cross correlation study is to be carried out between clinical, EEG and neuropathological data.

RESULTS

(a) GENERAL

Figure 1 shows the age distribution of the total population of 270. The median value for age is 49 years (52 years if the 24 children are excluded).

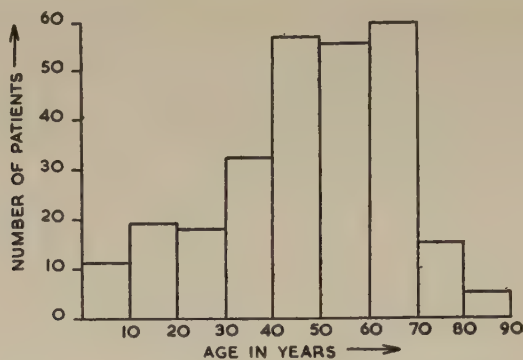


FIG. 1.—St. Faith's. Age distribution (270 patients).

In general, the patients come from large families with an average of 5 or 6 children and are of low social status. They are nearly all single (212 out of 246 adults) and the adults usually enter hospital when about 30 years old. They appear to be admitted at this time in their lives not so much because of worsening fits or deteriorating behaviour as for social reasons, quite often the death of the mother. The average time spent in hospital for the whole population is about 16 years (17·3 years if children are excluded).

(b) PHYSICAL FINDINGS

These are summarized in Table I. About one-third of the patients suffer

TABLE I
St. Faith's
Physical Findings (Gross)

	No. of Patients	Percentage No. of Patients
Paralyses	77	28·5
Other physical illness	26	9·6
No physical findings (gross)	167	61·9
Total	270	
Paralyses:		
Hemiplegia—Slight disability		
Right	22	
Left	16	
Moderate disability		
Right	5	
Left	7	
Severe disability		
Right	6	
Left	2	
Paraplegia	3	
Quadriplegia	3	
Other paralyses	13	

from physical incapacity and this includes 77 with one form of paralysis or another. There is a high incidence of hemiplegia and, although this can to some extent be accounted for by the fact that many of the patients are elderly, there

are also those in whom the hemiplegia either preceded or coincided with the onset of fits during infancy. Lack of documentary evidence precluded more detailed analysis but the assumption is that some of these cases are probably examples of the syndrome of hemiconvulsions, hemiplegia and epilepsy described by Vigouroux (1958) and by Gastaut, Poirier, Payan, Salamon, Toga and Vigouroux (1960).

(c) INFORMATION ON SEIZURES

In 37.2 per cent. of cases one or more relatives was thought to have had at least one seizure (Fig. 2). If only parents and siblings with a history of convulsions are considered then a "positive family history" is found in 26.3 per cent.

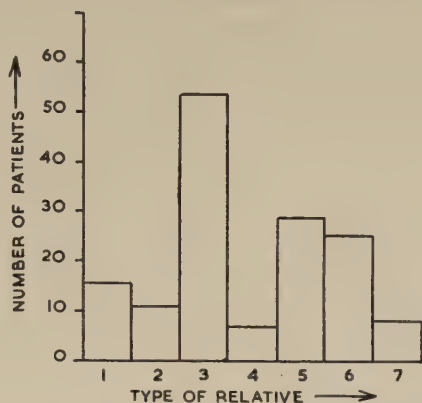


FIG. 2.—St. Faith's. Family history of epilepsy (270 patients).

1. Mother.
2. Father.
3. Siblings.
4. Children of siblings or patients.
5. Maternal relatives.
6. Paternal relatives.
7. ?Maternal or ?paternal relatives.

The total number of patients with a family history of fits is 100, or 37.2 per cent.

Figure 3 shows the age at the time of the first reported seizure. In nearly one-fifth of the patients this was during the first year of life.

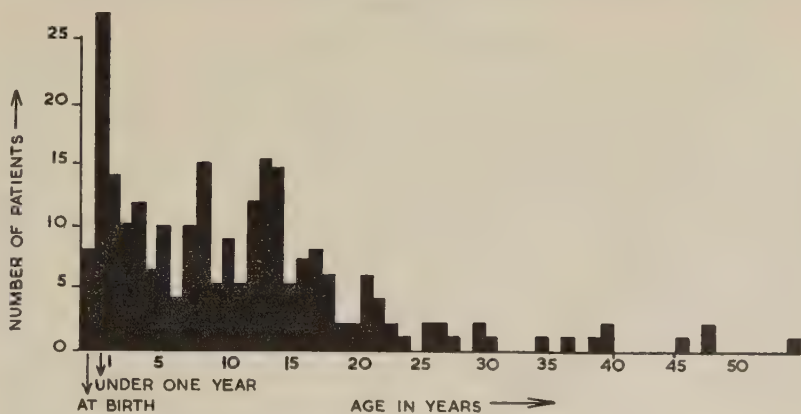


FIG. 3.—St. Faith's. Age of first seizure (270 patients).

Incidence of prodromata was based on statements by the nursing staff in respect of those patients of whom they felt it regularly possible to predict some hours previously that a fit would take place. There are nine cases not considered upon whom no nursing report was available but prodromes were described in 93 out of the remaining 261 (270-9). The different types of prodrome reported are given in Table II. Most commonly patients were noted as "irritable".

TABLE II
St. Faith's
Prodromata
(Total population sampled=261)

	No. of Patients
Frightened	1
Feels funny	3
Irritable	54
Sad	4
Wants to be alone	2
Quiet	2
Excited	2
Sings	1
Happy and laughs	2
Happy and sings	1
Irritable and sad	12
Irritable and wants to be alone	4
Irritable, wants to be alone and sad	3
Irritable and excited	1
Wants to be alone and sad	1
Total	93

Initial phenomena are listed in Table III. Of the patients with prodromata, 70 described "auras" and 23 denied them. Although Lennox (1960) and others,

TABLE III
St. Faith's
Initial Phenomena
(Total population sampled=270)

	No. of Patients
Sensory (mainly viscerosensory)	94
Psychical	14
Autonomic	1
Motor	10
Sensory and Psychical	22
Sensory and Autonomic	17
Sensory and Motor	20
Psychical and Autonomic	1
Motor and Autonomic	1
Sensory, Motor and Autonomic	1
Sensory, Psychical and Autonomic	1
Total	182

including the authors, use the term aura to embrace both sensory and motor events, the phrase initial phenomena has been used, following Penfield and Jasper (1954) in the absence of general agreement on nomenclature. This also avoids treating localizing motor disturbances as ictal while sensory and other happenings are regarded as somehow pre-ictal, any such distinction being considered unreal. One hundred and eighty-two patients reported initial phenomena, 74 denied any such and there were 14 from whom accounts could not be obtained.

Post-ictal phenomena of various types are shown in Table IV. Sleepiness

TABLE IV

*St. Faith's**Post Ictal Phenomena*

(Total population sampled=270)

										No. of Patients
Headache	..	..	..	..	..	..	..	..	..	153
Sleepy	..	..	..	..	..	..	..	..	..	171
Sick	..	..	..	..	..	..	..	..	..	16
Automatism	..	..	..	..	..	..	..	..	..	88
Difficulty in speech	..	..	..	..	..	..	..	..	..	36
Motor disability	..	..	..	..	..	..	..	..	..	22
Other phenomena	..	..	..	..	..	..	..	..	..	11
Mood swing	..	..	..	..	..	..	..	..	..	62

and a dull bi-fronto-temporal headache were by far the commonest post-ictal complaints. Most patients denied being incontinent during fits and tongue biting was likewise infrequent. Post-ictal automatism was fairly common as also was mood-swing, the latter usually towards increased cheerfulness. Most of the patients with post-ictal mood swings reported that they did not "feel right" until after they had had a "proper fit". The motor disabilities referred to in the table appeared in most cases to be based on a post-ictal paralysis but hardly any were examined immediately after fits so that a more ambiguous heading has been preferred.

*(d) TEMPORAL LOBE EPILEPSY**Classification from Clinical Findings*

Of the 270 patients, 24 children were not considered for classification purposes and from the 246 adults, 42 were eliminated on the grounds of inadequate clinical and/or EEG data. In the remaining sample of 204 adult female epileptics, 159 patients were classified as temporal lobe epileptics and 23 as not temporal lobe epileptics by both clinicians. There was disagreement in respect of 22 cases. This measure of agreement between the clinicians is significant at the 0.0005 level ($\chi^2=72.98$ for one degree of freedom).

If clinical criteria of assessment are adopted for the purpose of compiling incidence figures then 77.9 per cent. of the sample of 204 may be described as temporal lobe epileptics and comprise those patients concerning whom both clinicians are in agreement with respect to such a diagnosis.

Classification from EEG Data

If Bingley's (1958) criteria are accepted and classification based upon the EEG findings in epileptics without regard to the clinical details of the fit patterns, then 152 out of the 204 show EEG evidence in favour of temporal lobe epilepsy, i.e. the occurrence of focal spikes or sharp waves in the anterior and/or middle and posterior portions of the temporal lobe. The incidence figure based on this EEG criterion is 74·5 per cent.

Classification Based on Combined Clinical and EEG Information

The fact that incidence figures, whether derived from clinical or from EEG assessments, are so similar when expressed as percentages must not be assumed to support either or both forms of classification. The measure of agreement between the clinical findings and the EEG evidence for or against temporal lobe epilepsy was therefore tested and found to be significant at the 0·05 level ($\chi^2=7\cdot1$ for 2 degrees of freedom). Concordant clinical and EEG data in favour of temporal lobe epilepsy was found in 124 out of 204 cases. The incidence figure based on clinical and EEG agreement is correspondingly reduced to 60·8 per cent.

Finally, those cases in which in addition to focal temporal spikes or sharp waves, a localized reduction in barbiturate induced fast activity was seen between the sphenoidal and mid-temporal electrodes on the same side as the spike focus and in which also a clinical diagnosis of temporal lobe epilepsy was made independently by both clinicians, amount to 33·8 per cent.

All the above incidence figures have been set out in Table V.

TABLE V

*St. Faith's**Incidence of Temporal Lobe Epilepsy According to Different Criteria*

(Expressed as percentages of the "enough evidence group" of 204 adult females)

	Percentage No. of Patients
1. Both clinicians agree on a clinical diagnosis of T.L.E. 	77·9
2. EEG data compatible with T.L.E. 	74·5
3. Concordant clinical and EEG evidence of T.L.E. 	60·8
3a. Patients in group 3, whose EEGs also show localized diminution in barbiturate induced fast activity 	33·8

(e) NEUROPATHOLOGICAL DATA

Since the start of the project five years ago autopsy material has become available on about 30 patients from St. Faith's Hospital. Only provisional data are available and, although lesions of the uncus and hippocampal region are found in some 30–40 per cent. of cases, greater numbers will be required before much reliance can be placed on the incidence figure. Ammon's horn sclerosis, when present, is almost always unilateral. Small focal macroscopic lesions as described by Cavanagh (1958) have not been found to date.

DISCUSSION

Corsellis and Meyer (1958) in a neuropathological review of epilepsy and the temporal lobe pointed out that "this is a subject in which considerable activity is taking place and while so much remains unknown the constant supply

of information must inevitably result in a state of flux". The problem of how to supply useful information during this "state of flux" is not easily solved but an attempt has been made to follow the approach laid down by Symonds (1954) in his outstanding contribution on the classification of epilepsies. Symonds stated that "the necessity should be admitted for different categories of data" and suggested that presentation should be such that correlations might be established between the different categories but that the arrangement should also "provide opportunities for recording differences as well as similarities, the differences being equally important".

It was intended to base the incidence figure for temporal lobe epilepsy in this patient population on concordant clinical and EEG data separately collected and independently assessed. This gives a figure of 60·8 per cent. which, it is suggested, would generally be regarded as a conservative estimate. The question arises whether figures of 77·9 per cent. (combined clinical assessment) and/or 74·5 per cent. (EEG assessment) are to be preferred as more comparable with those of other series in which clinical, EEG, or mixed clinical and EEG criteria have been used. More important is the fact that different values have been found within the same study, dependent upon the different categories of evidence admitted.

The data which have been presented show that, while two clinicians may reach a high level of agreement with respect to independent clinical diagnosis and while there is apparently a relationship between clinical and EEG categories, the amount of concordance is indeed limited. There seems a parallel here with attempts to decide upon what clinical basis a diagnosis of syphilis may be placed on a positive Wassermann reaction in the absence of clinical signs or symptoms. In so far as clinical opinion is concerned, it is clear that some cases are more confidently diagnosed than others and the degree of confidence depends on the differential weighting applied to the different clinical findings. Gowers (1901), for instance, elected to accept an olfactory aura as the only basis for the diagnosis of uncinat epilepsy. Such a view would now be generally regarded as much too limited, but most clinicians would rightly be more impressed by the localizing value of an olfactory aura than by that of subjective vertigo or even by that of an epigastric sensation. Some would go further, and Penfield and Jasper (1954) state that "olfactory auras are comparatively rare and, in our experience, have often been associated with tumours of the temporal lobe in an anterior and inferior position".

There appear to have been two opposing tendencies, the one ever widening the notion of temporal lobe epilepsy in the direction of Fulton's physiological concept of limbic lobe epilepsy, and the other trying to break down the multiple symptoms and to allocate anatomical specificity to the several seizure components. Fuster (1953) made a considerable contribution in the latter direction. The fact is, however, that there is no great measure of agreement between different studies. Williams (1956) has for instance reported that fear "when appearing as an emotional experience and as part of the attack" was found in 70 per cent. of 50 patients with *anterior* temporal foci, but Gibbs (1958) has suggested that "attacks of fear, though rare, are more or less peculiar to patients with a *mid-temporal* focus".

The problem of the specificity of diagnostic criteria for temporal lobe epilepsy is not only academic. The practical consequences of such a diagnosis may be radical in so far as the patient is concerned because temporal lobectomy is often carried out as the treatment of choice on patients diagnosed as suffering from temporal lobe epilepsy and in whom anti-convulsant therapy has failed

to control seizures. The fact that excellent therapeutic results have been obtained in a significant proportion of cases, while gratifying, does not of course confirm the diagnosis any more than response to electroconvulsive or other therapy confirms a diagnosis of depression.

The particular value of the Maudsley series lies in the additional evidence which has been obtained from detailed neuropathological study of those portions of temporal lobes resected at operation. Falconer, Hill, Meyer and Wilson (1958) have pointed out that post-operative improvement was striking in the sub-group of tumours and glial hamartomas and marked in the sub-group of disseminated lesions with Ammon's horn sclerosis but that "results in the sub-group of disseminated lesions without Ammon's horn sclerosis were on the whole much poorer". Pre- and post-operative drug regimes were not described but it is not inconceivable that if anti-convulsant medication is maintained more or less constant any epileptic may benefit to some degree from the removal of a substantial portion of cerebral tissue. An alternative way of interpreting these results is to regard the group of cases, and they amounted to 15 out of 50, in which neither focal macroscopic lesions nor Ammon's horn sclerosis were found, as cases which should not be classified as suffering from temporal lobe epilepsy. Specificity of diagnostic criteria would, by these tokens, be dependent on the correlation between clinical, EEG and neuropathological evidence. This admittedly involves a return to the Jacksonian concept of a clinico-pathological association to which should now be added at least the third dimension of EEG findings. In our present state of ignorance, and given that the therapeutic implications of the diagnostic label "temporal lobe epilepsy" may involve a neurosurgical operation, it seems not unreasonable to test both EEG and clinical criteria against the neuropathological findings. This involves accepting the limitations of present neuropathological techniques but avoids the more dangerous assumption that failure to find localized pathology is due to these limitations. In course of time the biochemist may come to our aid.

Kennedy and Hill (1958) established a predictive value for the observation of unilateral localized reduction in barbiturate-induced (intravenous thiopentone) fast activity from sphenoidal to ear electrodes in patients from the Maudsley series. Specimens of temporal lobe tissue from those patients in whose pre-operative EEG records such an observation was made were likely to show lesions in the uncus and Ammon's horn on the same side as that upon which reduction of fast activity had been noted. The fact is, however, that barbiturate-induced fast activity can show local diminution in amplitude following a focal seizure and probably also in association with any considerable amount of focal spike activity even in the absence of a recent clinical seizure. In serial records from one patient it is possible to observe less barbiturate-induced fast activity on one side than the other in the temporal regions on one occasion but not on another. It is quite clear that a great deal still requires to be known about the significance and standards of measurement of localized reduction in barbiturate-induced fast activity. However, in view of Kennedy and Hill's (1958) findings, and notwithstanding the limitations which have been mentioned, it seems reasonable to treat the observation of reduced fast activity as an additional EEG criterion and indeed as an essential part of EEG evidence in favour of temporal lobe epilepsy. If this is done, then the incidence figure for temporal lobe epilepsy in the present series is further reduced to 33·8 per cent. It is of interest that this figure is of the same order as that for the observation of temporal lobe lesions in those cases which have come to autopsy.

The progressive narrowing of diagnostic criteria has been accompanied by a parallel decrease in incidence figures. Which figures are to be accepted is clearly a matter of opinion, but when the criteria are strictest the findings are more likely to be in keeping with the neuropathological evidence. It is not suggested that wider concepts are erroneous, and it is appreciated that they may ultimately prove more fruitful. On the other hand it must be recognized that the clinical diagnosis of temporal lobe epilepsy, as commonly made, represents a rather vague formulation. Patients with this form of epilepsy, however defined, may show a rhythmicity of seizure incidence, a greater tendency to have a "positive" family history than is seen in the general population and a liability for fits to be made better or worse by emotional and metabolic factors which must call seriously into question the view that these seizures necessarily start in the temporal lobe, or within the limbic system.

In conclusion it must be emphasized that among this population of long-stay female epileptics a fairly large group are considered likely to have unilateral temporal lobe lesions and might therefore be expected to benefit from temporal lobectomy. A controlled study on the effects of the operation is required in order to establish this. A long-term hospital population might well prove most suitable for such a study.

SUMMARY

The results of a survey into the incidence of temporal lobe epilepsy among a hospital population of long-stay female epileptics are presented. The incidence figures are shown to be different according to the categories of clinical and/or EEG evidence admitted. It is suggested that there is a lack of specificity of diagnostic criteria and reasons are given for proposing that the term temporal lobe epilepsy should at present be confined to describe those cases in which there is reason to suspect an association between clinical, EEG and neuropathological findings. It is further argued that if EEG classification is made stricter by admission of an additional category of data the possibility of correlation between EEG and neuropathological evidence may be increased as suggested by Kennedy and Hill (1958). Figures for the incidence of lesions of the temporal lobe, based on provisional autopsy findings, are offered in support of this view.

ACKNOWLEDGMENTS

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PSYCHOSIS OCCURRING IN THE SENIUM

A REVIEW OF AN INDUSTRIAL POPULATION

By

A. C. GIBSON

ROTH and his associates (1952, 1955, 1956) making observations on the classification and prognosis of psychotic illness occurring in elderly people found that the majority of patients fell into five main diagnostic categories; about half were cases of affective psychosis, most of which responded to E.C.T., and the rest suffered from arteriosclerotic and senile psychosis, paraphrenia and delirium. All the groups, with the exception of the senile psychotics, showed a considerable proportion of physical illness, particularly the cases of affective psychosis whose first attacks occurred after the age of sixty years. Most of the cases of delirium were physically ill and of the paraphrenics 30 per cent. were either blind or deaf.

This figure of 50 per cent. of an aged population suffering from affective psychosis is higher than those quoted in other papers written on this subject; Robertson and Mason Browne found that 33 per cent. of their cases were affective: Post also found that one-third of 214 aged patients admitted to an observation ward suffered from functional as distinct from organic psychosis. Camargo and Preston found that no less than 85 per cent. of 683 admissions to an American State Hospital were organic. The report of the Registrar General on mental illness shows that in 1952 and 1953, 27 per cent. of admissions over the age of 65 to mental hospitals in England and Wales suffered from manic-depressive psychosis or involutional melancholia.

These differences can be explained in different ways. It is possible that since the earlier papers were written, the pattern of psychosis in the elderly has changed, owing to altering social conditions, or because people look and behave as if they were younger than their years, if judged by the standards of 20 years ago. It could be that Roth's sample was not typical of the country as a whole, or that his criteria for diagnosis were not the same as those used in other studies. His cases were drawn from the relatively prosperous area of West Sussex where the slightly increased proportion of Social Class 1 and 2 might relatively increase the affective cases, but this seems unlikely to have any considerable effect. The high incidence of physical illness that he reported could be related to the fact that in a prosperous area where housing and living conditions are good one might expect that only the most severely incapacitated individuals would be hospitalised, and in this way an amount of physical illness might be found among the old people which was not representative of the population as a whole. It was therefore thought important to examine a group of elderly psychotics drawn mainly from an industrial district to see if the proportions of the diagnostic groups were substantially different, and whether the mental illness was often accompanied by a physical one.

One hundred consecutive admissions to the two mental hospitals serving the city of Newcastle upon Tyne and the town of Gateshead were examined. These hospitals also include in their catchment areas a part of north-west Durham concerned with coal mining and iron and steel manufacture, and a large,

though sparsely populated, agricultural area of Northumberland. Only 4 per cent. of the admissions came from the agricultural area, the remainder from the industrialised parts. A note was made of the social class from which the patients came: excluding the affective group practically all the patients were from Classes 4 and 5. Of the 45 affectives, one came from Social Class 1, three from Class 2, 15 from Class 3, and 13 each from Classes 4 and 5.

Some previous studies are open to criticism in that the observations contained in them were made in retrospect from examination of the case notes. In the present study each patient was personally examined, both mentally and physically. A social history was obtained in every case. In cases other than those with affective illness or paraphrenia an E.E.G. was performed as a further aid to diagnosis. A chest X-ray, E.C.G., E.S.R., urine examination including microscopy, haemoglobin estimation and full blood count, and W.R. and Kahn tests were done in every instance.

RESULTS

The criteria for diagnosis were the same as those used in previous studies: a slight degree of intellectual deterioration was disregarded if there was considerable depression and usually these cases when they had received E.C.T. showed an improvement in intellectual performance due to increased spontaneity. Paraphrenia was diagnosed when a well-organized delusional system, with or without auditory hallucinations existed in the setting of a well-preserved personality and affective response. Delirium was evidenced by an abrupt onset of clouding of consciousness in a setting of a relatively well-preserved personality and intellect. Cases of dementia with a fluctuating course, emotional lability, insight or evidence of having suffered a cerebrovascular accident were considered arteriosclerotic, but if the course was steadily downhill without the above features the case was considered to be one of senile psychosis.

The material breaks up as follows: nine cases came into the *miscellaneous category*—of these three were paranoid schizophrenics of twenty or more years standing who were periodically admitted to hospital; of the remaining six, one was an epileptic, another a case of G.P.I., another a life-long hysteric with a transient amnesia, and three were cases of pre-senile dementia—one had Huntington's Chorea, one Alzheimer's Disease and one Jacob-Creutzfeld's Disease.

Affective Group

Of the other 91 cases, 45 had an affective disorder, 43 being depressed and two hypomanic. Of these 45 no less than 28 had some physical illness, viz.:

AFFECTIVE GROUP

First Attack Occurring Before the Age of 60 Years

Sex	Age	Type of Illness	Signs
F	71	Endogenous	Bundle branch block
M	69	Endogenous	Old posterior myocardial infarct
F	66	Hypomania	Bilateral corneal opacities
F	64	Involutional	Bilateral cataracts. Diabetic
F	66	Involutional	Precipitated by shingles
M	62	Endogenous	Precipitated by shingles
M	65	Endogenous	Bilateral exophthalmos. Mediastinal cyst
M	64	Endogenous	Tertiary syphilis
F	72	Involutional	Paralysis agitans

Fourteen other cases had no physical signs.

First Attack Occurring After the Age of 60 Years

Sex	Age	Type of Illness	Signs
F	63	Involutional	Gross osteoarthritis of knees. Old myocardial infarction.
M	67	Endogenous	Depressed since myocardial infarction in December, 1956
F	73	Involutional	Auricular fibrillation. Left ventricular hypertrophy. Cataracts. Bundle branch block
F	68	Involutional	Diabetic. Angina of effort. Aortic stenosis. Left ventricular hypertrophy
F	74	Endogenous	Blood pressure 200/130. Left ventricular hypertrophy. Posterior infarction
F	64	Hypochondriacal	Blind. Deaf. Urinary infection. Rectal prolapse. Bundle branch block
M	73	Involutional	Old posterior infarct
M	69	Hypochondriacal	Coronary thrombosis since discharge from hospital.
F	72	Endogenous	Recent myocardial infarction.
F	68	Endogenous	Old septal infarction
M	83	Involutional	Pulsus alternans. Prostatism and inverted T waves
F	75	Involutional	Diabetic. Right cataract. Gangrenous toe. Bundle branch block.
F	71	Endogenous	Right eye blind. Cataract.
M	72	Hypomaniac	Gross kyphoscoliosis. Acute bronchitis. Cor pulmonale
M	74	Involutional	Emphysema and chronic bronchitis
M	70	Hypochondriacal	Chronic bronchitis. Enlarged prostate.
M	84	Endogenous	Left hemiparesis

Five other cases had no physical signs.

It may be seen that in those cases which develop their first attack of depression after the age of 60 a physical illness is a far more constant concomitant. (If Chi-square is applied, the difference between the two groups is significant at the 1 per cent. level.) More remarkable is the fact that 12 of these 22 cases had either clinical or electrocardiographic evidence of coronary artery disease. It may be thought that this is not such an unusual incidence in an elderly population, but if one compares it with the 55 cases in this study who were not affectively ill one finds that only eight of these had evidence of coronary artery disease, and that four of these occurred in the group of arteriosclerotic psychosis. This group is strictly comparable, as none of them had been admitted to a mental hospital before the age of 60 years. This difference is significant at the 1 per cent. level. Of this affective group a year after admission four had died, 33 were well, and eight were in-patients, four only being re-admissions for relapse of their illness.

Arteriosclerotic Psychosis

This group comprised 14 cases, eight female and six male. Eleven of these had some evidence of a cerebrovascular accident, six had evidence of renal involvement, seven had hypertension and four coronary artery disease. A year after admission five had died, five were in-patients and four were at home and reasonably well.

Senile Psychosis

This group comprised only 11 cases, all of whom were females. Six of this group showed some physical illness, three had auricular fibrillation, one was

deaf, another diabetic and a sixth had a large goitre. Nine of the eleven were dead a year after admission.

Paraphrenia

This group consisted of ten cases, six women and four men. There was ample evidence of the sensory deprivation said to occur with this disease for three were deaf, one blind and one deaf and blind. A year after admission five were still in-patients, three had been discharged (relieved) and two were dead.

Delirium

This group was unusual in that it contained a higher proportion of very old people than one would have expected. It may be seen from the following table that six of the cases were over 80.

Sex	Age	Signs	Result
M	83	Heart failure	Died within two days of admission
F	81	Heart failure	Died of pneumonia
M	67	Uraemia due to pyelonephritis. Auricular fibrillation. Subacute combined degeneration of the cord	Died of uraemia
M	85	Deaf. Carcinoma of the bronchus	Died
F	77	Deaf. Renal tumour with secondaries	Died
F	83	Melaena. Congestive cardiac failure	Died
M	81	Recurrent pneumonia plus urinary infection	Recovered
M	84	Urinary infection secondary to carcinoma of bladder	Died
F	70	Pneumonia	Recovered
F	68	Retro-peritoneal abscess	Died
F	63	No physical illness discovered	Recovered

DISCUSSION

These findings are essentially the same as those found by Roth, though the patients are drawn from an industrial area. Forty-five per cent. had affective illness and 33 of these were well a year after hospital admission. The cases of delirium either died or recovered; half the paraphrenics were still in hospital; the cases of senile psychosis were nearly all dead within a year and of the arteriosclerotics about one-third were dead, one-third improved and one-third the same. Therefore the incidence of functional and organic psychosis and their prognosis is the same in this series as in those published by Roth and one can scarcely doubt the validity of his diagnostic criteria, as the outcome of his group of affective psychotics was so favourable. The high proportion of sensory deprivation occurring in paraphrenics was confirmed. More careful physical investigation has revealed what seems to be an alarmingly high proportion of coronary artery disease in affective psychosis, particularly as we treat these people with E.C.T. Most physicians would agree that providing myocardial infarction has not occurred within the previous month or two coronary atheroma is not a contraindication to treatment. There seems no correlation between the types of affective disorder and the physical illness. Previous studies showed that physical illness is commoner in elderly psychotic males than females, which is a reversal of the usual sex distribution, and that it is commoner

in the late than the early onset group. These phenomena were observed in the present study, and suggest strongly that the physical illness has some part in the precipitation of the psychosis. It would of course be wrong to suggest that it is the whole "cause"—old men get depression without physical disease, and the depression often continues when the physical condition has subsided. Roth and Kay have shown statistical evidence that in certain psychiatric illness in old age physical illnesses have some share in causation. In the confusional or delirious states they are probably specific causes, being both necessary and adequate to produce the illness. In the affective disorders they certainly contribute to causation and the same situation exists in relation to deafness among the paraphrenic cases. It is surely worth our while to direct every effort to treating the physical as well as the mental aspects of illness in elderly mental hospital in-patients.

SUMMARY

One hundred consecutive admissions to two mental hospitals serving mainly industrial Tyneside were examined mentally and physically. Of these 45 had affective psychosis, 14 arteriosclerotic psychosis, nine paraphrenia, 11 senile psychosis, 11 delirium and 10 miscellaneous conditions. There was no loading of Social Classes 1 and 2 to increase the proportion of affective cases. The affective cases, particularly those who developed their first attack after the age of 60, showed a high incidence of coronary artery disease, and the cases of paraphrenia a high incidence of sensory deprivation. Follow-up a year after admission showed the outcome of the different diagnostic categories to be similar to that previously described.

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RELEVANCE OF BASE RATES TO THE VALIDITY OF A PSYCHOLOGICAL TEST OF BRAIN DAMAGE (M.W.-L.T.)

By

DONALD WALTON

In a recent paper by Meehl and Rosen (1955) several important criticisms are made against the majority of diagnostic psychological instruments reported in the literature. Some of these criticisms are well known, for example absence of cross-validation data, not stating the type of population most suited for the particular psychometric device, etc. The main purpose of the present paper is to try to meet, with respect to the M.W.-L.T. (Walton and Black, 1957), one of their other major criticisms namely, that "Base rates are virtually never reported. It is, therefore, difficult to determine whether or not a given device results in a greater number of correct decisions than would be possible solely on the basis of the rates from previous experience. When, however, the base rates can be estimated, the reported claims of efficiency of psychometric instruments are often seen to be without foundation."

Base Rates refer to actuarial or experience tables, that is, they refer to the percentage of patients or subjects in a given population who are found, from experience, to be suffering from certain symptoms or characteristics. In their paper Meehl and Rosen report actual studies to illustrate the importance of base rates. One of these was an investigation by Hanvik (1949) into some of the psychological dimensions associated with low-back pain. In this study, a psychological test was used to differentiate those patients with psychogenic and organic low-back pain. The test correctly identified, on cross-validation, 70 per cent. of those patients with no organic findings (they scored above a critical cut-off point), whilst 70 per cent. of the "organic" low-back pain patients scored below this optimum cut-off point. Meehl and Rosen continue, "Assume that 90 per cent. of all patients in the Neurology Service with a primary complaint of low-back pain are in fact 'organic'. Without any scale at all the psychologist can say every case is organic, and be right 90 per cent. of the time . . . with the scale . . . of 10 psychogenic cases, 7 score above the line; of 90 organic cases, 63 score below the cutting line. If every case above the line is called psychogenic, only 7 of 34 will be classified correctly or about 21 per cent. Nobody wants to be right only one out of five times in this type of situation, so that it is obvious that it would be imprudent to call a patient psychogenic on the basis of the scale. Radically different results occur in prediction for cases below the cutting line. Of 66 cases 63, or 95 per cent., are correctly classified as organic. Now the psychologist has increased his diagnostic hits from 90 to 95 per cent. on the condition that he labels only cases falling below the line, and ignores the 34 per cent. scoring above the line."

As a further illustration Meehl and Rosen consider what the value of the above test would be if it were found that of patients with negative neurological findings, 90 per cent. were diagnosed as psychogenic and 10 per cent. as organic. This would clearly impose further limitations on interpretations derived from the scale. The scale would now correctly classify 95 per cent. of all cases above

the line as psychogenic but would misclassify 27 of the 34 cases below the line. It would similarly be imprudent of a psychologist to interpret low scores in such a situation.

In actual fact, the psychologist would of course most likely not test every low-back pain case. Probably those referred would be a select group. And so the population would naturally change from "all patients in neurology with a primary complaint of low-back pain" to "all patients in neurology with a primary complaint of low-back pain *who are referred for testing*".

Since the practical value of the M.W.-L.T. appears to depend in fact upon base rates, information was sought as to the base rates of patients who had been referred by the psychiatrist to the psychologists for testing, because it was considered that the patients might be suffering from brain damage. The psychologist in these instances was always asked to give an opinion with regard to the presence or absence of brain-damage.

The information was gained by an examination of the files of patients who satisfied the above criteria and who had been referred for testing during a two-year period. Two previously published predictive studies cover this period (Walton and Black, 1959; Walton and Mather, 1960). One anomaly must be pointed out. Whilst the 1960 study was entirely undertaken at Winwick Hospital, the 1959 study was completed both at Winwick Hospital and nearby Rainhill Hospital. There is no reason to believe, however, that the populations tested differed significantly, though because of this anomaly the two studies will first be treated separately and then combined.

RESULTS

Table I shows the results of the "1959 study" in terms of base rates of the population tested and the diagnostic groupings of this population according to the M.W.-L.T. The style of the table follows very closely that advocated by Meehl and Rosen (1955).

Of the 28 non-brain-damaged patients, none score at or above the optimum cut-off point of 26; of 29 organic cases, 19 score at or above this point, 10 scoring below it. Thus of the 38 patients who score at or below the critical cut-off point, 28 or 74 per cent. are not brain-damaged. Of those 19 patients scoring at or above the cut-off point all were subsequently diagnosed as brain-damaged. In other words the test has increased the valid positive scores from 51 per cent. to 100 per cent. on the condition that only patients scoring at or above a score of 26 are labelled as brain-damaged. The use of the test with patients scoring below the cut-off point also produces valid results in excess of those provided by the above base rates. High scores appear, however, to be more positive evidence of brain damage than low scores are of an absence of brain damage.

TABLE I
1959 Study
Actual Diagnosis

Classification by M.W.-L.T.		Not Brain-Damaged	Brain-Damaged	Total Classified by Scale
Base rates in population tested: 51% brain-damaged; 49% not brain-damaged				
Not brain-damaged ..		28	10	38
Brain-damaged ..		0	19	19
Total diagnosed ..	..	28	29	57

Table II contains the results of the "1960 study". Of the 11 non-brain-damaged patients 9 scored below the cut-off point; of 29 organic cases, 23 scored above the line and 6 below it. Thus of the 25 patients who score above the crucial cut-off score, 23 (92 per cent.) are in fact finally diagnosed as brain damaged, a discriminatory rate far in excess of the revised base rates contained at the top of Table II. When it comes to the fifteen patients scoring below the line, only 9 (60 per cent.) are in fact finally diagnosed as not brain-damaged. It is obvious therefore that more caution needs to be observed in interpreting scores below the cut-off point.

TABLE II
1960 Study
Actual Diagnosis

Classification by M.W.-L.T.		Not Brain-Damaged	Brain-Damaged	Total Classified by Scale
Base rates in population tested: 73 % brain-damaged: 27 % not brain-damaged				
Not brain-damaged ..	..	9	6	15
Brain-damaged ..	..	2	23	25
Total diagnosed ..	..	11	29	40
				(1 patient excluded here because of catastrophic re- action.)

From an examination of the combined studies (Table III) the M.W.-L.T. correctly identifies 95 per cent. of all cases scoring above the line as brain-damaged (42 of 44), and is correct in identifying 70 per cent. of all cases below the line as not brain-damaged (37 of 53). It appears that the psychologist can

TABLE III
1959 and 1960 Studies Combined
Actual Diagnosis

Classification by the M.W.-L.T.		Not Brain-Damaged	Brain-Damaged	Total Classified by Scale
Base rates in population tested: 60 % brain-damaged: 40 % not brain-damaged				
Not brain-damaged ..	..	37	16	53
Brain-damaged ..	..	2	42	44
Total diagnosed ..	..	39	58	97

increase the number of his "diagnostic hits" by the use of the M.W.-L.T. in the clinically doubtful case of brain damage, and interpret with very considerable confidence high scores as being indicative of brain damage. This is confirmed by an analysis of the data according to the Meehl and Rosen formula (1955):

$$P(o) = \frac{Ph. 1}{Ph. 1 + Qp 2}$$

This formula is used to estimate, "the probability that an individual is diseased (brain-damaged) given that his observed test score is positive (high)". According to the data contained in the 1959 study, one would be correct in making such

decisions, in the long run, 100 per cent. of the time; according to the 1960 study 91 per cent. of the time, whilst according to the combined studies in 95 per cent. of the time.

Several times in a previous paper it has been stressed that the extent of the predictive validity of the M.W.-L.T. depends on the percentage of patients included in the organic sample who might be suffering from generalized as opposed to localized cerebral damage (Walton and Mather, 1960). Very high predictive test validity has been demonstrated when "localized organics" have been excluded. It was decided, therefore, to examine the 1959 and 1960 base rates of those patients referred for psychological testing and whose final diagnosis was either not brain-damaged or was brain-damage of the type likely to affect the whole cortex (senile dementia, cerebral arteriosclerosis, G.P.I. disseminated sclerosis, etc.). Tables IV, V and VI provide the results.

TABLE IV
1959 Study

Classification by the M.W.-L.T.	Actual Diagnosis		Total Classified by Scale
	Not Brain-Damaged	Brain-Damaged	
Base rates in population tested: 44 %generalized brain-damage: 56 %not brain-damaged			
Not brain-damaged ..	28	4	32
Brain-damaged ..	0	18	18
Total diagnosed ..	28	22	50

TABLE V
1960 Study

Classification by the M.W.-L.T.	Actual Diagnosis		Total Classified by Scale
	Not Brain-Damaged	Brain-Damaged (generalized)	
Base rates in population tested: 61 % generalized brain-damage: 39 % not brain-damaged			
Not brain-damaged ..	9	2	11
Brain-damaged ..	2	15	17
Total diagnosed ..	11	17	28

TABLE VI
1959 and 1960 Studies Combined

Classification by the M.W.-L.T.	Actual Diagnosis		Total Classified by Scale
	Not Brain-Damaged	Brain-Damaged (generalized)	
Base rates in population tested: 50 % generalized brain-damage: 50 % not brain-damaged			
Not brain-damaged ..	37	6	43
Brain-damaged ..	2	33	35
Total diagnosed ..	39	39	78

It is apparent from Tables IV to VI that should the M.W.-L.T. be restricted in use to diagnostic problems associated with generalized cortical involvement the number of misclassifications as contained in Tables I to III would be reduced very considerably. The results illustrated in these Tables also emphasize the marked superiority of the test to base-rate predictions. As reference to Table VI will show, as many as 86 per cent. of those with scores below the cut-off point were in fact not brain-damaged (37 of 43), whilst 94 per cent. of those with scores above the cutting-score turned out to be brain-damaged (33 of 35). Very similar pictures are painted by Tables IV and V.

The results therefore suggest:

1. When the psychologist is dealing with psychiatric referrals, the latter aimed at elucidating the presence or absence of brain damage, and this population is undifferentiated with respect to the type of brain damage which might be included (that is, it is likely to include organic patients with localized cerebral lesions as well as those suffering from generalized cortical pathology), then one can speak with very considerable confidence that high positive scores (26 or above) are indicative of brain damage. One can speak with less confidence that a low score rules out the possibility of brain damage, though low scores (25 or below) are seen to occur in a high percentage of cases which are not brain-damaged.

2. When, however, the psychologist is dealing with psychiatric referrals, the brain-damaged patients from which are probably suffering from generalized rather than localized organic pathology, he can speak with far more confidence that a low score argues against generalized cortical damage. On the other hand a high score would be strong, positive evidence of brain damage.

SUMMARY

The results from two predictive studies of the validity of a psychological test of brain damage (M.W.-L.T.) were compared with the number of correct decisions which would be possible solely from the rates from previous experience (base-rates). The results emphasize the superiority of the test over base-rate predictions and so obviate it from the type of criticism put forward by Meehl and Rosen with respect to the reported validity of the majority of diagnostic psychological instruments reported in the literature.

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JUVENILE AMAUROTIC IDIOCY:

A CASE REPORT

By

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THE first clinical histories of this condition were reported in Norway by Stengel (1826). Batten (1903) described changes in the macula in two members of a family who had undergone progressive mental deterioration with loss of visual and motor functions. Vogt (1906) published similar cases and attempted to delimit them as a juvenile form of Tay Sachs disease. It differed from the infantile form in that it occurred in childhood after a period of normal development; the rate of progression was slower; it was not limited to Jewish children and the ophthalmological appearances were those of optic atrophy. The following year Spielmeyer described the pathological changes in juvenile Tay Sachs disease as essentially similar to the infantile form. Further detailed pathological studies were made by Greenfield and Holmes (1925).

Sjögren (1931) collected 59 cases from a "Swedish rural population". He found the natural history of the condition uniform and highly characteristic. Blindness occurred between five to eight years, the ophthalmological appearances being those of retinitis pigmentosa with optic atrophy and dense pigmentation around the macula. Between eight and twelve years behaviour disorders, intellectual deterioration and epileptiform fits occurred. In the final stage profound dementia with pyramidal and extra-pyramidal syndromes developed. Death ensued at about 18 years of age.

Sjögren considered the condition dependent on a recessive hereditary factor. Like other rare recessive traits in man it is extremely irregularly distributed (Penrose, 1961). It has been most commonly reported in Sweden.

Juvenile amaurotic idiocy implies a psychological picture of severe subnormality. Clinical descriptions stress a progressive intellectual decline occurring in previously normal children. The term dementia would be more accurate and Mott (1907) suggested the condition be called "amaurotic dementia".

The following case report has a typical clinical history; it is of special interest from the genetic, haematological, and electrophysiological aspects.

CASE HISTORY

Ian, aged 16 years, was referred to Dr. Denis Hill from the Royal School for the Blind at Leatherhead, for investigation of epileptiform fits.

Family History. The parents are not Jewish and not consanguineous. Mother, aged 51 years, is a placid, intelligent woman of pyknic physique. Father, aged 49 years, is healthy and works as a builder.

Siblings. (i) David—aged 25 years, is married and is an industrial chemist. He became "short-sighted" at 12 years: when examined ophthalmoscopically no abnormality was found other than a refractive error. (ii) Leonard—aged 22 years, single, in good health, works as a sales representative. (iii) John—aged 9 years, is a high-grade mongol child who is able to wash, dress and attend to his own toilet.

There is no previous family history of epilepsy, blindness or subnormality.

Personal History. The patient was born in 1944; pregnancy and delivery were normal. He sat unsupported at 7 months, stood at 12 months and was continent of urine and faeces

at two years. His early development was normal and he attended school from $4\frac{1}{2}$ to $9\frac{1}{2}$ becoming competent at reading, writing and arithmetic. School reports described him as "above average" up to the age of 8 years. He had mild attacks of measles and chicken pox between 4 and 5 years.

Present Illness. Poor vision was suspected at $8\frac{1}{2}$ years when he began to misjudge the distance of objects and to have difficulty in reading. After failure of refractive methods to improve his vision he was referred to Moorfields Eye Hospital where a diagnosis of Batten's disease was made. He attended a school for "partially sighted" children from 5-10 years, but throughout his adjustment was poor. He was unable to read Braille and made no progress at arithmetic. The parents felt that he deteriorated intellectually and became unable to undertake simple arithmetical problems he had been capable of at 9 years. More recent attempts at academic training have had to be abandoned and he can now do only simple basket making.

The first epileptiform fit, consisting of a generalized tonic-clonic convulsion, occurred at 12 years. No history of aura or prodromata could be obtained. Attacks recurred three to four times per year. Minor attacks consisting of transient lapses of consciousness were observed to occur with the same frequency.

His speech became progressively more indistinct from 14 years due to marked pressure of talk and a tendency to "run" words together with perseveration of syllables.

Physical Examination. He was of asthenic physique and walked in a stiff, stooping manner with short steps. This gait is characteristic of blind people and not specific to this condition as Sjögren considered.

Central vision was absent but he was able to appreciate large, moving objects in the lower visual fields. Examination of the optic fundi showed patchy pigmentation throughout the retina of both eyes with dense areas around the macula. The optic disc was "waxy" in colour and the retinal vessels were small. A tremor of the left hand and arm was present but there was no alteration of tone. Plantar reflexes were flexor. Liver and spleen were not palpable.

Mental State. He was a pleasant, cheerful youth who would frequently attempt to perform tasks beyond his visual capacity. He became irritable if restrained or frustrated. He confabulated when asked to describe objects and persons in his vicinity. Orientation was correct, and memory for remote events was good. Retention and recall of recent events was poor. He was only able to carry out simple addition and subtraction; spelling was inaccurate beyond three letter words. There were no delusions or hallucinations.

Investigations. Hb. 90 per cent., E.S.R. 3 mm. Liver function tests were normal. Wassermann reaction in blood negative. X-ray of skull was normal apart from some thickening of the left squamous temporal bone. Examination of peripheral blood smears (Jenner-Giemsa) showed vacuolation in 50 per cent. of lymphocytes (Fig. 1).

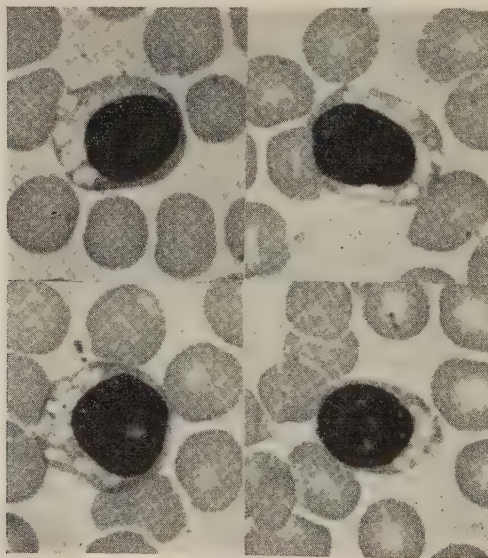


FIG. 1.

EEG (Fig. 2). Two scalp recordings and a sphenoidal EEG (with wire electrodes) were carried out.

Prominent 8 cycle per second activity was seen but showed anti-phase further forward than is usual with normal alpha. Eye opening and closure had little effect. There was a diffuse excess of 4-7 cycle per second activity which from time to time showed sudden fluctuations in voltage.

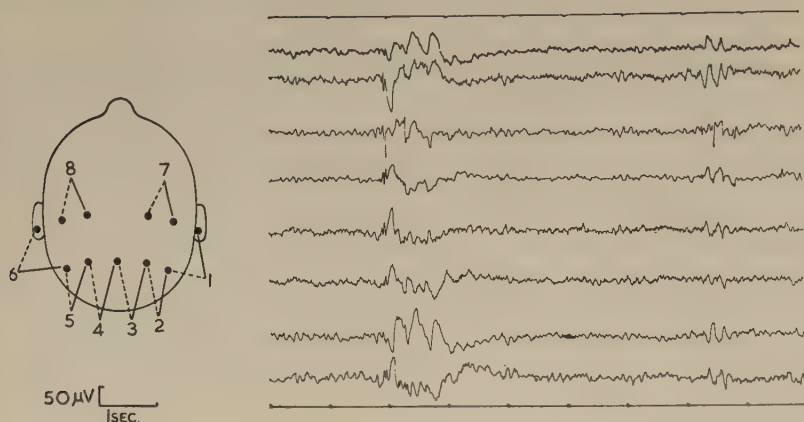


FIG. 2.

On this background more clearly defined paroxysmal discharges occurred. They varied in shape and frequency from sinusoidal frontal delta at 3 cycles per second to complexes of mixed slow, intermediate slow, sharp waves and spikes whose location was more variable. They appeared to arise "out of" 8 cycle per second activity. There was a suggestion of pseudo-periodicity. The paroxysmal discharges were considered to be of subcortical origin; their occurrence rate was not affected either by hyperventilation or photic stimulation.

Psychological Tests. A Wechsler Verbal Scale administered in 1956 gave an I.Q. of 86: repeat of this test in 1960 was 73. The Williams' Intelligence Test for children with defective vision gave an I.Q. of 78. Both Wechsler and Williams' tests are heavily loaded with verbal material and thus assess intelligence in a narrow sense. On the basis that the patient was described as a "bright" child and his previous Wechsler score was 86, the results support the clinical evidence of general intellectual deterioration.

DISCUSSION

The first systematic study of the EEG in neuronal lipidosis was made by Cobb, Martin and Pampiglione (1952). A common pattern was recognized in twelve cases. The background activity consisted of irregular, slow and intermediate slow activity of high voltage. On this background irregularly recurring bilaterally synchronous high voltage triphasic waves of less than 0.2 second duration were seen. The authors suggested there was some degree of specificity for the EEG in cerebral lipidosis.

Crawley (1957) made serial EEG studies in three cases of juvenile amaurotic idiocy and showed increasing abnormality in the EEG with progression of the clinical state. This represented the electrical response to progressive destruction of cerebral neurones and no specific diagnostic value could be attached to the EEG. Similar conclusions were reached by Morrell and Torres (1960) in serial EEG studies on a case of infantile Tay Sachs disease.

The EEG in Ian's case was similar to that described by Crawley. There was a suggestion of pseudoperiodicity in the occurrence of the paroxysmal discharges but it did not reach the level seen in inclusion body encephalitis. It seems unlikely that EEG specificity can be claimed in cerebral lipidosis.

The occurrence of juvenile amaurotic idiocy and mongolism in consecutive siblings is of genetic interest. Abnormalities in chromosomal karyotype have

been confirmed in mongols (Lejeune *et al.*, 1959; Ford *et al.*, 1959) whilst cases of "familial mongolism" can be due to translocation mechanisms (Polani, 1960). In view of the occurrence of a mongol sibling it seemed possible that the patient might have an abnormal karyotype. A skin biopsy, culture and examination of the chromosomes constitution was carried out by Professor Penrose. The karyotype however was that of a normal male.

Vacuolation in the lymphocytes of cases of juvenile amaurotic idiocy was first described by van Bagh and Hortling (1948). The findings were confirmed by Nissen (1954) and Tegljaerg (1955) who also found vacuolation in the lymphocytes of other normal members of the families. Mittwoch (1959) has reported cytoplasmic inclusions in the lymphocytes of six cases of gargoylism. In two cases vacuolation similar to that described in juvenile amaurotic idiocy occurred. Two types of vacuoles were observed by Jelgersma (1960) in the lymphocytes of a brother and sister with juvenile amaurotic idiocy. The first type had large, circular, sharp-edged vacuoles; the second type were smaller with a more irregular edge. The blood of all the normal relatives, except the mother, showed vacuoles of the second type in the lymphocytes. This could be a manifestation of the gene for juvenile amaurotic idiocy in heterozygotes. Rayner (1952) has suggested the possibility of detecting the heterozygotes by this simple method.

Abnormal vacuolation was easily seen in 50 per cent. of the lymphocytes of peripheral blood of our case. Blood films were made from a finger prick using the coverslip technique and stained with Jenner-Giemsa. Blood films of all members of the family including the mongol sibling were examined by Professor Davidson, but no convincing abnormal vacuolation was present in the lymphocytes.

Vacuolation in the lymphocytes of cases of juvenile amaurotic idiocy is possibly due to deposition of lipid and claims to have detected abnormal neutral fats by cytochemical methods have been made (Tegljaerg, 1955). This phenomenon is not restricted to cases of juvenile amaurotic idiocy since it has been noted in gargoylism, infantile Tay Sachs disease (Spiegel-Adolf, 1959) and Niemann-Pick's disease (Crocker, 1958). However vacuolation may have some specific diagnostic value in neuronal lipidosis and be useful for the detection of cases in large clinical populations. Full reporting of this phenomenon in cases of neuronal lipidosis would help to establish its degree of specificity.

SUMMARY

A case of juvenile amaurotic idiocy is described; a younger sibling is a mongol. Vacuolation of lymphocytes was seen in peripheral blood smears; no abnormality was seen in the lymphocytes of other members of the family. This observation may have some diagnostic value in neuronal lipidosis.

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FREQUENCY OF BLOOD TYPES IN A HOMOGENEOUS GROUP OF MANIC-DEPRESSIVE PATIENTS*

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THE aim of this study was to determine the incidence of selected blood types in a homogeneously diagnosed group of manic-depressive patients as compared to the incidence of these blood types in a control group and in reported figures for the general population. This interest in the blood characteristics of manic-depressive subjects was stimulated by the finding of a relatively high incidence of active duodenal ulcer or medically and roentgenographically confirmed duodenal ulcer histories in manic-depressive patients (16 per cent.) (12) as contrasted with the reported incidence of duodenal ulcer in the general population (5-10 per cent.) (4). The specific association of duodenal ulcer with O-type blood has been reported by several investigators (1, 2). Although the reported relationships between blood type and specific disease entities have been critically questioned in a recent review, the reviewer concluded that the evidence "tends to suggest that a relationship between duodenal ulcer and group O blood actually exists" (10). The known and suspected relationships of blood types with various disease processes have been summarized by established investigators in the field of blood grouping and classification (14).

Psychiatric research concerned with the study of emotional disorders and the epidemiological investigation of these disorders has been handicapped by the lack of well-defined and consistently applied diagnostic criteria. Thomas and Hewitt (17) in an extensive and detailed survey of the literature in 1939, justly criticized studies of the blood type in physical and mental disease up to that time because of inadequate hematological technique and statistical analysis. The results of their own studies, using more sophisticated methodology, nevertheless were consistent with the findings of most previous studies which failed to reveal any differences between normal and mentally ill persons for ABO, MN, AO and A₂B blood groups. But Thomas and Hewitt, as most investigators in this area before them, gave neither clear diagnostic criteria nor behavioural descriptions for the different diagnostic groups they compared. In fact, they explicitly note "... some patients were difficult to place, so that the primary state on admission to the hospital was used for analysis in preference to the state at the time of determination (of blood samples)".

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Although in recent years there have been very few investigations of the relationship between blood types and psychiatric disorders, a study in the United States in 1957 reported a greater number of group A₁ blood types for a large sample of schizophrenic patients as compared to normal controls (6). This report did not describe adequately the patient group or define the diagnostic criteria used.

In psychiatry, as in communicable diseases, the disorder itself is only a limited expression of an extremely complex epidemiological (and ecological) problem. Yet, an understanding of the details of the chain of events which ultimately results in the manifest reaction of man to his environment may lead to the discovery of the weak links in the chain, thus making it possible to interrupt or modify it at some point, and thereby prevent or reduce the extent of the occurrence of the manifest disorder. But the epidemiologist, as has been noted by Leymaster (9), must have the assurance that he is dealing with a relatively unified condition as far as clinical manifestation and prognosis are concerned if he is to control a communicable disease. At the present time in psychiatry, however, our scientific efforts are limited by inadequate descriptions and conceptualizations of the entities studied. Therefore, in order to circumvent these present deficiencies there is need for consistent and detailed operational specification of the phenomena to be investigated. This implies that diagnostic definitions for each disease category are the necessary first step in any investigation (15). In the present study, the disease categories have been given behavioural and historical definitions. Only time can demonstrate the usefulness of these definitions, but it is hoped that they will facilitate the comparison of the findings of the present investigation with later work.

DIAGNOSTIC CRITERIA

Diagnostic concepts of manic-depressive reactions have been reviewed by our group (12). Our interests in various aspects of this disorder and our dissatisfaction with the lack of clarity in these diagnostic concepts led to a series of studies (2, 13, 3) which have attempted to differentiate psychopathological groups. Whenever possible and appropriate in these studies, control groups of psychoneurotic depressive patients or schizophrenic patients were also evaluated. In the present study, a psychoneurotic depressive group was judged to be the appropriate control. This control group was selected because the cyclic aspects of manic-depressive reactions have been emphasized in genetic studies of this disorder (5) and the discovery of differences in the blood-type characteristics of manic-depressives as compared to psychoneurotic depressives might suggest mechanisms of genetic transmission. The criteria utilized for the selection of the manic-depressive and control subjects included in this study are summarized below:

Manic-depressives

Manic-depressive patients were diagnosed essentially according to the criteria defined by Kallmann and utilized by him in his genetic studies. Subjects were "... restricted to cyclic cases which have shown periodicity of acute self-limiting illness before the fifth decade of life, and no progressive or residual personality deterioration before or after psychiatric episodes of elation or depression" (5). The criteria of the present study also required that either the depression or elation had to be sufficiently disabling to require hospital care and treatment. Furthermore, no patient was included in the manic-depressive

group who possessed more than one of the signs found to be pathognomonic of schizophrenia by Lewis and Piotrowski (8) in a follow-up of patients previously diagnosed as manic-depressives. The manic depressives in the present studies were all white males. Seventy-three of them had been hospitalized at some time during the past six years at the V.A. Hospital or the Duke Medical Center, Durham, North Carolina. Thirteen were patients at the V.A. Hospital, Salisbury, North Carolina, when selected for the study. Each of the latter group was personally examined by the senior author and the history evaluated.

Psychoneurotic Depressives

These subjects were white male patients characterized by depression of mood with biological concomitants of depression. They did not meet the criteria for manic-depressive reactions and were negative for the Lewis and Piotrowski signs of schizophrenia. Most cases had past histories of chronic tension and neurotic physical complaints. The 67 psychoneurotic depressive patients were admissions to the V.A. Hospital, Durham, North Carolina, in the latter part of 1958 and in early 1959.

The manic-depressive and the psychoneurotic depressive subjects in the present study were predominantly of English, Scotch, and Scotch-Irish descent with a smaller number of partial German descent. The sample reflected the general ethnic origins of the white population of North Carolina. Although we were interested in obtaining Negro manic-depressive subjects, the number who met our diagnostic criteria was not large enough to allow reliable comparisons.

PROCEDURE

All blood typing was done by a single technician who had had extensive blood-typing laboratory experience, and who had not had any transfusion reactions due to errors in typing during her employment in a blood bank in the preceding five years. The blood groups were determined by standard control methods described by the Ortho Manual (1955) and Race and Sanger (14) for ABO and AB types, Kell, Duffy, MN reactions, and Rh factors DCEc. To check the reliability of the hematological procedures, repeat samples under different numbers for 34 of the subjects were submitted for retyping when these patients were seen on follow-up visits.

Lewis *a* reactions were determined on approximately one-half the subjects and *s* type reactions on about one-third; but, because of difficulties in obtaining reliable antisera for these rare blood types and the absence of significant trends, both were discontinued. Electrophoretic haemoglobin typings were determined by another laboratory engaged in haemoglobin research for 35 of the manic-depressive subjects and approximately one-half of the psychoneurotic patients. No abnormal trends or variations were observed except for one of the psychoneurotic depressive patients*.

RESULTS

Blood typing data from the manic-depressive and psychoneurotic depressive groups were compared with population figures for Caucasians (14, 15). The number and per cent. of manic-depressives and neurotic depressives evidencing

* Further investigation of this patient and his immediate family revealed the apparent origin of this variation—it resulted from inter-marriage with an American Indian.

the various blood characteristics compared to the per cent. incidence of these blood types in the general Caucasian population are shown in Table I*. The significance of the differences in blood characteristics for the manic-depressive group as compared to the general population and to the psychoneurotic depressives is evaluated by Chi Square analysis in Table II. The incidence of type-O blood was significantly greater ($p < .001$) in the manic-depressive group

TABLE I

Blood Characteristics of Manic-Depressives as Compared to a Psychoneurotic-Depressive Control Group and General Population Norms

Blood Characteristics			Manic-Depressives		Psychoneurotic Depressives		General Population
			N	%	N	%	%
Type	O	..	51	59	27	40	40*
	A	..	26	30	34	51	45
	B	..	4	5	5	7.5	10
	AB	..	5	6	1	1.5	5
MN	+-	..	24	28	22	33	30†
	-+	..	16	19	19	28	22
	++	..	46	53	26	39	48
Duffy (Fy(a))	+	..	60	70	42	63	66†
	-	..	26	30	25	37	34
Kell	+	..	12	14	10	15	9†
	-	..	74	86	57	85	91
D	+	..	74	86	59	88	85*
	-	..	12	14	8	12	15
C	+	..	62	72	44	66	70*
	-	..	24	28	23	34	30
E	+	..	27	31	33	49	30*
	-	..	59	69	34	51	70
c	+	..	74	86	59	88	85*
	-	..	12	14	8	12	15

* Levinson, S. A., and McFate, R. P., 1956, "Approximate per cent incidence in Caucasians".

† Race, R. R., and Sanger, Ruth, 1958, "English white populations".

than in the general Caucasian population (15)†. Similarly, the incidence of O-type blood was greater for the manic-depressive group than for the psychoneurotic depressive group ($p < .05$). None of the other blood type differences between the manic-depressive patients and the general Caucasian population reached a level of statistical significance.

* The ABO determinations were available for a total of 90 manic-depressive patients; 60 per cent. were found to have O-type blood. Data for 86 of these were reported in Table I as only those patients with complete typing of all the blood groups studied are represented in the table.

† Levinson and McFate's norms were used in this comparison to population figures because they were more complete for the ABO groups. Large series of blood group distributions have not been reported in our geographic area on non-donor populations. When compared to Sanger's norms (16) in which the incidence of type-O blood is 43 per cent., the Chi Square was 9.3 and the significance level was $P < .01$.

TABLE II

Summary of Chi Square Analysis of the Significance of the Differences in the Incidence of Blood Types for Manic-Depressives Compared to Psychoneurotic Depressives and General Population Norms

Blood Characteristics	Manic-Depressives v. General Population	Manic-Depressives v. Psychoneurotic Depressives†
O v. Other (A, B, AB) ..	14.06†	4.71*
MN	1.23	3.81
Duffy	.47	.56
Kell	2.21	.00
D	.09	.02
C	.22	.46
E	.06	4.32*
c̄	.09	.02

* $p < .05$

† $p < .001$.

‡ Yates' correction was applied for all computations based on 2×2 contingency tables.

The number and per cent. of manic-depressive subjects with the more common Rh phenotype combinations were compared with psychoneurotic depressives and available population norms (11) in Table III where it may be noted that the manic-depressive percentages closely approximate the population norms. The high incidence of E positives (49 per cent.) in the psychoneurotic depressive group may also be noted in this table. The incidence of Rh factor E was significantly greater for the psychoneurotic depressive group (see Table I, II) than for either the manic-depressives ($p < .05$) or the general population ($\chi^2 = 12.05$; $p < .001$).

TABLE III

Rh Phenotype Frequencies of Manic-Depressives as Compared to a Psychoneurotic Depressive Control Group and General Population Norms

Rh Phenotype						Manic-Depressives		Psychoneurotic Depressives		General Population*
D	C	E	c̄			N	%	N	%	%
+	+	-	+	..	..	35	41	20	30	34.9
+	+	-	-	..	..	12	14	7	10	18.5
-	-	-	+	..	..	10	12	6	9	15.1
+	-	+	+	..	..	14	16	16	24	14.1
+	+	+	+	..	..	13	15	15	22	13.4
+	-	-	+	..	..	0	0	0	0	2.1
-	-	+	+	..	..	0	0	1	1.5	0.9
-	+	-	+	..	..	2	2	1	1.5	0.8
+	+	+	-	..	..	0	0	1	1.5	—

* Mollison, *et al.*, 1952.

Since the subjects in the present study were predominantly native to North Carolina or the South-eastern U.S.A., there was the possibility that regional differences in blood characteristics might be found. Furthermore, depression which was common to both groups of subjects in the present study might theoretically have blood-type correlates. Therefore, the data for the manic-depressives and psychoneurotic depressives were combined and compared with the appropriate population norms. The incidences of type-O blood for the combined group (51 per cent.) was significantly greater than for the general population ($\chi^2 = 7.88$; $p < .01$). As may be seen in Table I, this difference was

due obviously to the high incidence of type-O blood for the manic-depressive patients.

The only other blood characteristic for which the combined sample differed from the population norms was on the Kell factor. The incidence of positive Kells for the combined sample was 14.4 per cent. as compared to 9 per cent. for the general population ($\chi^2=5.03$: $p<.05$). It may be noted in Table I that both the manic-depressive and psychoneurotic depressive subjects contributed about equally to the Kell difference.

DISCUSSION

The findings of this investigation substantiated the hypothesis derived from our earlier observation that the incidence of O-type blood was greater for white manic-depressive subjects than for either white psychoneurotic depressive subjects or the general Caucasian population. This finding supports the impression of a possible genetic factor in manic-depressive illness emphasized by Kallmann, Slater and others (5) and indicated in our previous report (12). Unanswered questions are posed as to the possible biological or chance relationship of peptic ulcer to manic-depressive reaction or pathological mood changes. Intriguing, but at present speculative, is a possible theoretical relationship between type-O blood group, including genetic factors, and the psychodynamic oral and alimentary problems that one observes in both manic-depressive and peptic ulcer patients.

Not predicted was the 49 per cent. E positive reactions in the psychoneurotic depressive group (Table I). That this was not likely to be due to a local population difference was suggested by the fact that the incidence of such reactions for the manic-depressives closely approximated the population norms. Whether or not E positive blood is a characteristic of patients with psychoneurotic depressive reactions or merely a chance finding needs to be evaluated by determinations in other samples of psychoneurotic subjects similarly diagnosed and compared with local control groups.

The findings of a higher incidence of positive Kell reaction in the present study was also unexpected. This finding, if substantiated by further work, could be interpreted as reflecting either a higher incidence of this blood characteristic in North Carolina (south-eastern U.S.A.) or a possible relationship between the Kell factor and depression. But, in either case, this finding has implications for the need to do routine Kell status determination for blood donors for prospective mothers in this geographic area. That the Kell factor may be one of the incompatibilities in the occurrence of icterus neonatorum has been documented (14).

CONCLUSIONS

Although the number of cases in the present study was not large, the cases were carefully selected on the basis of specified diagnostic criteria so as to yield clearly defined subject groups that could be easily replicated by other investigators. With the definitions used, it would appear that O-type blood occurs more frequently in manic-depressive patients than in either psychoneurotic depressive patients or the general white population. This finding also suggests that there may be endogenous constitutional differences between manic-depressives and psychoneurotic depressives and that the differences between these two reactions are not limited merely to the degree of depression. This supposition obviously

does not preclude the possibility that both reactions may be precipitated and/or accentuated by external environmental experiences.

Further confirmation of the relationship of particular blood types to manic-depressive reactions and psychoneurotic depressive reactions is needed. It would be desirable to repeat these studies with similarly defined subjects from other geographic areas. A study comparing blood types of non-hospitalized cyclothymes with manic-depressives, psychoneurotic depressives, and schizophrenic patients, as defined for the other aspects of our studies of manic-depressive reactions (12) is contemplated.

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NEUROLOGICAL CHANGES ASSOCIATED WITH P.A.S. AND I.N.A.H. THERAPY

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SHORTLY after isoniazid (I.N.A.H.) was first used in the treatment of tuberculosis, it became apparent that side-effects were not uncommon. Thompson (1952) lists euphoria, hyperreflexia, vertigo, muscular twitching, insomnia, paraesthesiae and transient loss of memory among the reactions observed. These were noted to be mild and reversible but did suggest involvement of the nervous system.

That peripheral neuritis can be caused by I.N.A.H. is now well recognized. Gammon, Burge and King (1953) reported the findings in 46 cases drawing attention to the relation between the rapidity of onset of the peripheral neuritis and the size of the dose. Biehl and Vilter (1954) pointed out that rapid regression of symptoms occurs if the drug is promptly withdrawn but that residual defects may persist for up to a year if administration of the drug is continued. Polyneuritis was also described by Mollaret and Dupont (1959) who emphasized a gradual onset with paraesthesiae and occasionally the subsequent development of paralysis of the lower motor-neurone type. Neuropathological studies are rare but Ott, Rabinowicz and Morand (1959) report one case and describe demyelination of the peripheral nerves with relatively few changes in the central nervous system.

However, functional changes in the activity of the central nervous system have been found following the administration of isoniazid. Reilly *et al.* (1953) obtained evidence that large doses of isoniazid lower the seizure threshold to photic stimulation in man. In addition psychiatric abnormalities have been described. The commonest such disturbance would appear to be the development of mental states in which euphoria is a main symptom (Hunter, 1952). It is not uncommon for other disturbances to occur. Jackson (1957), for example, described five cases each differing in their main psychiatric symptom; one presented as an hysterical reaction, one as a mania, one with anxiety and paranoid ideas, one with a "schizoid reaction" and one with depression. Hare (1958) described two cases whose illness in many ways resembled schizophrenia. Complete recovery usually occurs following cessation of the therapy, although in the two cases described by Hare, the symptoms persisted six months and two years after withdrawal of the drug. Further evidence that the central nervous system may be involved comes from a case reported by Biehl and Vilter (1954). Their patient was treated with 20 mg./kg. of I.N.A.H. per day, developed convulsions and died. Death was considered to be due to a toxic encephalopathy.

More recently isoniazid has been implicated in another syndrome, that of spastic paraplegia. Money (1960) has described three cases occurring in patients treated for between three and eighteen months on conventional doses 2 to 4 mg./Kg./day). Psychiatric abnormalities were not mentioned and there was no

evidence of peripheral neuritis. They recovered rapidly when administration of the drug was stopped and treatment with pyridoxin was commenced.

The two case reports that follow describe the main clinical features of two patients receiving treatment with isoniazid and para-amino salicylic acid, who developed definite disturbances in the central nervous system. In addition, both patients developed symptoms of an affective disorder.

Case 1

The patient (Miss L.) was born in 1909. The family history is not significant except that her brother has had two depressive illnesses treated in hospital. The patient took a B.A. degree in French and German when aged twenty and then taught these languages until 1939, when she became a censor of mail. Later she worked in Austria and in 1953 obtained a post as a research assistant in a National library.

Her previous health record was good and the only illness of note was an attack of depression lasting three months in 1950. This illness was associated with her father's terminal illness but she was not admitted to hospital for treatment.

Her friends and relatives described her as a quiet, studious person, fond of books and music. She was not a worrier and not subject to frequent mood changes. Her employers considered her a most able worker.

The patient was well in October, 1955, when she had a routine X-ray of the chest. This showed a small shadow in the right lung, but no tubercle bacilli were found in the sputum. She was treated with P.A.S. 10 Gm. a day, plus I.N.A.H. 200 mg. a day. There was a little diarrhoea, but she remained well for three weeks. Twenty-three days after starting the medication she developed general malaise and a widespread urticarial rash. The drugs were stopped a few days later. She developed a fever, she became confused and was admitted to Southfields Hospital, Edinburgh, under the care of Dr. F. W. A. Turnbull. On admission, there was a widespread urticarial rash, generalized lymph node enlargement and slight jaundice. Her temperature was 103·6° F., and she was disorientated in time and place. Two days later there was a sudden deterioration in her condition. She became comatose and was incontinent of faeces and urine, her arms were flexed and her legs were extended. She remained in this state for three weeks. Cerebro-spinal fluid examination was normal. Thereafter there was a slow and gradual improvement, associated with periods of disorientation. She found that she had difficulty in using common objects and could read and write only slowly. After six weeks it was possible for her to leave hospital but a few weeks later, because of depressive symptoms, she was admitted to the Maudsley Hospital under the care of Dr. Brian Ackner.

On admission, the only abnormal physical signs were to be found in the C.N.S. She walked stiffly and her movements were clumsy. Although right-handed, she tended to use her left hand when making fine movements. There was no evidence of sensory impairment. All limb reflexes were brisk; the abdominal and plantar reflexes were normal. There was no evidence of dysphasia; her knowledge of French and German was unimpaired. There was a right inferior quadrantanopia, constructional apraxia (block design), right-left disorientation and poor scores with simple mental arithmetic. These abnormalities suggested the presence of a left parietal lobe lesion. The EEG showed a minimal asymmetry, being of lower amplitude in the left parietal regions. Minimal slow activity was also present in the left posterior temporal areas.

She was a solitary woman, who often cried, and was very irritable. She slept poorly and there was early morning waking. She was severely depressed and contemplating suicide. Subjectively she experienced impairment of memory and there was a complete amnesia for January, 1956. More detailed psychological testing revealed a now average intelligence, whereas from her past academic record a superior level would be expected. Manual abilities showed evidence of definite impairment. (The Wechsler-Bellevue Verbal Scale—119; Performance Scale—89.)

She remained in hospital for five months, during which time a rehabilitation programme was arranged, aimed at improving her writing, typing and walking. Supportive psychotherapy was given, as were mild stimulant drugs. There was a slow general improvement and three months after admission depressive symptoms became less severe and improvement continued until she was well enough to leave hospital.

At home she remained fairly well for a few weeks, then there was a gradual return of the depressive symptoms. Suicidal ideas were particularly prominent in the mental state, and she was re-admitted to hospital after five months at home. On this occasion the physical signs were unchanged, and there was again evidence of a severe depressive illness. She was given 12 E.C.T.s, during the course of which there was a marked improvement in her condition, though she remained hypochondriacal and despairing about her disabilities. On this occasion she was able to leave hospital seven months after admission and was referred for a rehabilitation course which she was unable to complete. Follow-up five years after the initial illness showed that she was still not working, the main symptoms being inability to concentrate, slowness, and emotional lability.

Case 2

The second patient (Mrs. C.) was born in 1934, the eldest of five. There is no history of mental ill-health in the family. She attended primary school, where she was described as average, left school at fifteen and had a succession of office jobs. She married in 1955 and has one child, born in 1959.

Her previous mental and physical health was good apart from enuresis till the age of six. She is described as being a happy, sociable girl with few outside interests apart from dancing and the cinema. There were no episodes suggesting transient neurological disturbances.

In June, 1959, a routine antenatal chest X-ray revealed an opacity at the apex of the right lower lobe. Her E.S.R. was 36 and she was treated with isoniazid 300 mg. and P.A.S. 16 Gm. daily. She persisted with the treatment in spite of nausea and vomiting lasting several months. In April, 1960, she complained of weakness of the legs, but no abnormality was noted. She became depressed and irritable and attempted suicide with sedatives and was then admitted to Bethlem Royal Hospital under the care of Dr. E. H. Hare.

Physical examination on admission in July, 1960, revealed no abnormality apart from brisk but equal tendon reflexes, the jaw jerk was not exaggerated. Mentally she was depressed, retarded, unable to concentrate, but she was correctly orientated and there was no dysmnnesia. No improvement was obtained with 6 E.C.T.s. Gradually her condition deteriorated and she became withdrawn, incontinent of urine and faeces, and episodes of sweating and tachycardia without fever were noted. The patient then gradually developed hypertonicity of the legs, and left ankle clonus with an extensor plantar response were noted. There was no abnormality of the arms and no sensory impairment.

Special investigations: Hb. 83 per cent. E.S.R. 10. C.S.F. and Lange normal. Chest, skull and spine radiographs were normal. The EEG showed no significant abnormality.

In October, 1960, P.A.S. and I.N.A.H. were withdrawn and an injection of a multiple vitamin preparation (Parentrovite) given daily along with 100 mg. of pyridoxin orally.

Over the next two weeks her mental state improved so that she was able to co-operate more fully. Examination revealed disorientation in time and space, impairment of memory, more especially for recent events, and complete amnesia for the period since her admission. She was able to perform simple mental arithmetic. The cranial nerves were normal, as were the arms. There was no sensory impairment. Spastic paraplegia was present. Physiotherapy was commenced with gradually increasing co-operation of the patient. By the middle of December, 1960, slight movement was noticed in the left hamstrings, quadriceps and extensors of the toes. By January, 1961, her mental state was normal apart from amnesia for most of her hospital admission. Movements were present now in most muscle groups of both legs. There has been uninterrupted recovery until the present time, when the patient is attempting to walk.

There was at no time evidence of a dissemination of the lesion suggestive of multiple sclerosis, although this cannot be excluded with certainty. Likewise, at no time was any sensory impairment observed although this again may have been present as a transient disturbance when the patient was unco-operative.

DISCUSSION

Both these patients while receiving isoniazid and P.A.S. in normally accepted doses, developed illnesses in which there was definite involvement of the central nervous system. In the first patient it is evident that the main damage was suffered by the parietal cortex; this occurred in the setting of a generalized toxic reaction. The second patient showed signs of an upper motor neurone lesion though the site of this must remain in doubt. The spastic paraplegia would indicate a level below the cervical enlargement; the briskness of the deep reflexes in the upper limbs would suggest a higher level, but the absence of an exaggerated jaw reflex makes it likely that the lesion occurred in the spinal cord.

Both patients showed an affective disorder with marked depressive features; in the first patient, the depressive features were persistent, but in the second patient seemed to occur as part of a toxic psychosis. It appears to us that these neurological changes were due to P.A.S. and I.N.A.H. acting either singly or in combination. Neurotoxicity to P.A.S. is rare (Mollaret and Dupont, 1959), although delirium has been recorded (Alenquer, 1958) also polyneuritis (Brun and Vialler, 1951). However, it is possible that it may cause anorexia and vomiting as in the second patient and thereby reduce the absorption of vitamins from the intestine.

It is probable that the reactions that occurred in these two patients were due to I.N.A.H., although it must be emphasized that this cannot be proven. The similarity in structure between isoniazid and nicotinamide might suggest that it would block the action of this substance. Biehl and Vilter (1957) pointed out, however, that no case of pellagra had been observed following I.N.A.H. treatment. They could demonstrate no disturbance of nicotinic acid metabolites in the urine and a high urinary pyridoxin excretion was found in patients given large doses of I.N.A.H. These workers also noted the protective action of pyridoxin in preventing the peripheral neuritis frequently observed. The mechanism by which I.N.A.H. blocks the action of pyridoxin would seem to be by the formation of physiologically inactive compounds by the two substances (Greenfield, 1958).

It is of interest that in multiple vitamin deficiencies, pyridoxin specifically improved the insomnia, irritability, muscular weakness and difficulty in walking (Spies *et al.*, 1939). In experimental deprivation of the vitamin in a human subject, mental depression and confusion were noted by Hawkins and Barsky (1948). In monkeys, experimental deprivation of pyridoxin produced degenerative changes in the large pyramidal cells of the cortex and one animal showed loss of medullated fibres in both pyramidal tracts in the cord (Victor and Adams, 1956). Thus the similarity between the effects of pyridoxin deprivation and the toxicity of isoniazid is striking. Tower (1956) points out that pyridoxin is essential for the activity of a number of important enzyme systems in the metabolism of the brain and that it would also seem to be essential for the maintenance of the neural lipids in myelin.

It is of interest that both these patients developed severe depressive symptoms while receiving isoniazid though the chemically related iproniazid was the first of the compounds now known as the mono-amine oxidase inhibitors that have been shown to be of some value in relieving depressive states (Kline *et al.*, 1958; Robie, 1958).

Although rare and predominantly reversible, toxic reactions to isoniazid can be severe and perhaps permanent. The variety of their manifestations may well lead to their being misdiagnosed. In view of the experimental work mentioned above, treatment should include the administration of pyridoxin.

SUMMARY

Case histories are presented of two patients who developed neurological changes whilst receiving isoniazid and P.A.S. The relevant literature is reviewed and the possible mode of action of the drugs in producing these changes are discussed.

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IMIPRAMINE (TOFRANIL) IN DEPRESSIVE STATES

A CONTROLLED TRIAL WITH IN-PATIENTS

By

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and

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INTRODUCTION

A LARGE number of papers has been published in the last two or three years on the use of imipramine in depressive states. Much of this literature has been reviewed by Ball and Kiloh (1959) whose investigations on out-patients represents one of the remarkably few controlled trials that have so far appeared. We feel, therefore, that there is room for a report on another controlled study, this time with in-patients, in which the term "controlled" not only involved the usual "blind" procedures of administering tablets, but also included some of those elementary safeguards of reliability and validity of assessments without which uncertainty must remain concerning the meaning of any findings. Foulds (1958), for example, has presented evidence that *uncontrolled* investigations have a greater tendency to yield results supporting the hypotheses of investigations than experimentally fully *controlled* trials. In view of the claims that had been made for the efficacy of imipramine in a number of uncontrolled studies, we were primarily concerned to discover whether or not there existed valid scientific evidence of its value and, if so, in what kinds of depressive illness it appeared to be of most benefit.

MATERIAL AND METHODS

Over a five-month period consecutive admissions to Springfield Hospital of cases of depression were randomly assigned to imipramine and placebo groups. The placebo tablets were indistinguishable in appearance from the genuine ones, and the hospital pharmacist alone knew which type of tablet each patient was having. Shortly after admission each patient was interviewed and assessed independently by two of us (C.F. and M. de M.), and only when both agreed on the diagnosis of primary depression was the patient included in the series; patients in whom depressive symptoms were thought to be secondary to a schizophrenic or organic illness were excluded. In addition, a very few cases where the doctor in charge of the patient felt it would be ethically unjustifiable to withhold E.C.T. were also excluded.

Diagnosis was based on the patient's history and the clinical interview. We agreed with Lehman and others (1959) that observation, interview and history are the simplest and most reliable tools in the psychiatric diagnosis of depression, and that at present there exist no objective clinical tests on which we can base a diagnosis. Features regarded as being diagnostically significant were subjective or objective depression of mood, psychomotor retardation or agitation, feelings of guilt or unworthiness, self-reproach, suicidal ideas, insomnia with early morning waking, diurnal variation of mood, indecisiveness, and loss of interest and ability to concentrate.

In addition to the clinical interview, each patient was given a questionnaire

designed to elicit the degree of behavioural and subjective depression. This questionnaire was constructed to serve as an outside criterion of depression—as a validity check—from items culled from several sources, including the Minnesota Multiphasic Personality Inventory. It contained 25 items to which patients had to reply either “True” or “False”. (Norms were available from a control study.) The following are examples of the items in the questionnaire:

“I often feel a hopeless failure.”

“I often have feelings of unworthiness.”

“I feel worse in the morning.”

“I have lost interest in many things lately.”

Each patient was also rated independently on two rating scales. The first of these was a 5-point scale based on that of Shapiro *et al.* (1958) to assess the degree of depression clinically present. A rating of 2 would be given if the clinician found “Depression without gross depression in the patient’s appearance”, and a rating of 4 was given if the clinician found “Severe depression with delusions and hallucinations but in good contact”. The second rating scale was the depression sub-scale of the Wittenborn Psychiatric Rating Scale (Wittenborn, 1951). The items of this scale are specifically aimed at the degree of delusional guilt and unworthiness present, e.g. “Patient tends to blame himself or refer to his unworthiness” or “Patient appears to have a delusional belief that he is an extraordinarily evil, unworthy or guilty person”. The first item would be rated 2, and the second item would obtain a rating of 4.

The tablets were administered in a standardized dosage 25 mg. t.d.s. for 2 days, then 50 mg. t.d.s. for 2 days, then 75 mg. t.d.s. for 4 weeks. In the majority of cases the dosage was later reduced to 50 or 25 mg. t.d.s. when the patient had either improved or was showing side-effects which he could not tolerate.

The two rating scales and the questionnaire were re-administered at the end of the course of tablets. This was normally after 6 weeks, but in a number of cases the period of trial was shorter either because the patient became better and was discharged, or because his or her symptoms became so serious that it was considered ethically unjustifiable to keep the patient in the trial. A number of patients who had been on placebo were given a further trial on the genuine drug, following which the ratings and questionnaire were again repeated.

We feel that the traditional double-blind method is not applicable to an investigation of the pharmacotherapy of depression. When treatment is discontinued, depressive conditions do not necessarily recur when switching to a placebo, as would be expected for example in a substitution therapy (*viz.* B₁₂ in pernicious anaemia). In addition, in a trial such as ours conducted with in-patients, remission of symptoms leads to the patient’s discharge and hence a new variable is introduced into the situation.

Originally 67 patients were included in the investigation. Of these 17 dropped out because they either discharged themselves or had to be given alternative treatment. Of the remaining 50 patients, all completed at least 4 but more often 6 weeks on either placebo or imipramine, while 6 completed a full trial on imipramine as well as placebo. It was not found possible to obtain all the necessary questionnaire and rating data on testing or retesting for all these 50 patients because of precipitate discharges, the death of one patient, and various administrative reasons. Thus second assessments on the depression questionnaire were obtained for only 47 patients, one of the raters completed pre- and post-treatment rating on 49 patients, and the other 43. Pre- and post-

treatment ratings from both raters are available for 42 of the 50 patients. In the final analysis of the results, only those 36 cases are considered where the raters agreed in the direction of post-treatment changes.

We do not intend to discuss in this paper the question of the classification of depressive illness. Recent literature ranges from total rejection of sub-groups in depression, viewing depressive conditions as a continuum on a quantitative scale, to various sub-groupings according to assumed aetiology, physiopathology or psychodynamic concepts. There is, however, almost general agreement that for therapeutic purposes it is expedient to classify depression as endogenous, involutional and reactive. On the whole therapeutic tests have justified such a distinction (Roth, 1959).

We classified our cases as follows: cases were regarded as endogenous when all or some of the following features were present; history of previous attacks, marked diurnal variation of mood, marked psychomotor retardation, or evidence of hereditary factors; cases were regarded as involutional when the first attack of depression had occurred in the involutional period, and was associated particularly with agitation rather than retardation, and with obsessional personality traits; the remainder were regarded as reactive. According to these criteria the sample contained 26 endogenous cases, 11 involutionals, and 13 reactive depressions.

RESULTS

The formal hypothesis to be tested was that the drug imipramine would be more successful in effecting improvements in depressive disorders than a placebo despite the therapeutic efficacy of placebos that has been demonstrated in many studies. The hypothesis was confirmed. Our calculations are based on 36 cases in which the two raters were in agreement in their post-treatment ratings as to the direction of change of clinically observable depression (Shapiro *et al.*, 1958). Of these, 17 patients were actually receiving imipramine, while 19 were on placebo. The sex distribution was 9 men between the ages of 52 and 62 (mean age=56.9, S.D.=11.8) and 27 women between the ages of 32 and 76 (mean age=56.9, S.D.=16.1). The difference in ages between those receiving imipramine and those on placebo was not statistically significant, though the mean of the former group was somewhat higher.

TABLE I

				Imipramine	Placebo	Total
Improved	..	..	..	11	4	15
Not improved or worse			..	6	15	21
Total	..	..	..	17	19	36

Table I shows that patients on imipramine had a significantly greater chance of improving than patients on the placebo (chi-square=5.353, $p=0.02$ approx. with $df=1$).

TABLE II

				Endogenous	Involutional	Reactive	Total
Improved	..	..	..	6	3	2	11
Not improved or worse				3	1	2	6
Total	..	..	..	9	4	4	17

Considering now only the 17 patients receiving imipramine, Table II shows that endogenous and involutional depressions have a greater chance of benefiting from the drug than reactive depressions. For reasons already given, the total

number of patients involved in the investigation was small, and the numbers given in Table II are actually too small to carry out statistical evaluations. The trends are, however, fairly clear.

When examining the two raters' post-treatment ratings of clinically observable depression separately, it was found that a more substantial contribution to the positive finding was made by the ratings of one than by the other. With the Wittenborn Scale as a rating device no significant results were obtained either with cases where inter-rater agreement on improvement had been obtained or when taking the ratings of the two raters separately. Considering the 6 cases who were put on imipramine after showing no improvement on placebo, and on whom post-treatment ratings were available, 4 showed improvement while 2 did not respond.

DISCUSSION

The significant improvement following imipramine treatment, and the relative lack of improvement following placebo, shown by means of the rating scale evidence of depression (Shapiro *et al.*, 1958) was supported by the change in scores on the depression questionnaire.

TABLE III

Group	Assessment	N	Mean Scores on Depression Question- naire	S.D.	Range
Controls	—	26	6.62	4.05	2-20
Present total sample	Pre-treatment	50	15.50	4.12	5-23
Imipramine	Pre-treatment	23	15.87	3.97	5-23
Imipramine	Post-treatment	23	8.74	4.10	0-21
Placebo	Pre-treatment	24	16.50	4.14	10-23
Placebo	Post-treatment	24	13.79	4.06	0-22

Table III indicates (a) that both the groups used in the present study scored much higher on this device, aimed deliberately at behavioural and subjective evidence of depression, than a normal control group, (b) that the two randomly obtained imipramine and placebo groups were well equated for depression as measured by this questionnaire prior to differential medication, and (c) that the imipramine group's depression score fell significantly following treatment ($p < 0.01$), whereas the placebo group's scores did not fall significantly.

Evidence of the validity of the questionnaire is obtained not only from the great discrepancy between the mean scores on the scale of an un-hospitalized, heterogeneous sample of the population and the hospitalized sample, but also from the correlations between the questionnaire and the rating scales. These correlations, shown in Table IV, are all statistically significant at better than the 0.01 level.

TABLE IV

Correlations Between Questionnaire and Rating Scales

Questionnaire			Overt Depression (Shapiro <i>et al.</i> , 1958)	Wittenborn (1951)
Rater I:				
Pre-treatment	..	..	0.48	0.54
Post-treatment	..	..	0.77	0.69
Rater II:				
Pre-treatment	..	..	0.57	0.55
Post-treatment	..	..	0.74	0.78

The fact that the pre-treatment correlations are consistently lower than the post-treatment correlations might at first sight suggest that the questionnaire we were using is more valid as a post- than a pre-treatment measuring instrument. That this is a misleading impression is shown by the variations in inter-rater reliability in Table V.

TABLE V
Inter-Rater Reliability

				Scales	
				Overt Depression (Shapiro <i>et al.</i> , 1958)	Wittenborn (1951)
Pre-treatment	..	..	..	0.46	0.66
Post-treatment	..	..	..	0.75	0.62

These figures suggest that the apparently lower correlation between scores on the depression questionnaire and ratings on overt depression particularly, may be due in part to lower pre-treatment rating reliability. This suggests that the assessment of freedom from symptoms may be easier and less susceptible to subjective bias, than the assessment of the absolute severity of depression.

Our findings support those of other studies (e.g. Ball and Kiloh, 1959) which demonstrated the therapeutic effects of imipramine. In our study, improvement in all depressive states following imipramine is consistently about 65 per cent., taking each rater separately, or taking only those cases where both raters agree in the degree of improvement, stability or worsening of symptoms.

We feel it should be emphasized, however, that of 8 patients whose illnesses were characterized by the presence of frank delusions of guilt or unworthiness, and who were clearly the most severely ill cases in our series, none showed any significant response to Tofranil, while 5 of them subsequently responded to E.C.T.

Our experience in this investigation illustrates some of the difficulties inherent in therapeutic trials in psychiatric illness in general, and in depressive illness in particular. Firstly, the influence of psychological and interpersonal factors, which were quite distinct from any effects attributable to a drug, was clearly operating in some cases, and caused difficulty in assessing results. The methods of assessing patients were even themselves occasionally responsible for producing emotional changes in the patient, thus making rating difficult. Secondly, in carrying out a therapeutic trial on depressive illness ethical questions become particularly serious in view of the availability of a treatment, i.e. electroplexy, which is generally believed to produce rapid relief of symptoms in most cases. Doctors tend therefore to be unwilling sometimes to keep patients for weeks on a possibly ineffective tablet, particularly in severe cases where there is a risk of suicide.

With regard to side-effects, like other investigators we found that Tofranil produced in many patients an atropine-like effect of mild degree. Many patients complained of dryness of the mouth and this was more disturbing in patients with obsessional features. Many patients showed increased perspiration during the treatment period. Other symptoms complained of were dizziness, weakness, lassitude, lack of energy, feeling of pressure in the head, tremor of hands, disturbances of visual accommodation, and in one case an erythematous eruption. Side-effects were more pronounced during the first four or five days of treatment and receded as the treatment went on. We did not observe jaundice or any Parkinsonian symptoms. Because of the side-effects it is of psychological

interest that one patient developed increased perspiration and nausea and was then found to have been on placebo. It was also of interest that hypomania, a complication which has been reported by a number of observers, developed in one patient, and he too was on placebo. In one ward there was amongst the nursing staff and patients some degree of prejudice against Tofranil, and in this ward all the patients whether on the genuine drug or on the placebo developed side-effects for a time.

We think that it is extremely difficult to carry out controlled experimentation where the needs of the patients and the demands of scientifically valid procedure are equally satisfied. It may be considered that the present study, though based on relatively small numbers, at least took into account some of the more elementary validity and reliability safeguards, which are often absent from investigations claiming to have been "controlled".

SUMMARY AND CONCLUSIONS

1. A controlled trial of Tofranil in the treatment of 50 cases of depression admitted to Springfield Hospital is described.

2. In addition to the usual random distribution of cases into active drug and placebo groups, special reliability and validity safeguards were used.

3. Taking all cases of depression together, it was found that patients on Tofranil had a significantly greater chance of improving than patients on placebo.

4. Endogenous and involuntional depressions benefited more from the drug than reactive ones, but the most severe cases, whatever their classification, failed to respond to the drug.

5. Some of the difficulties inherent in therapeutic trials in depressive illness are discussed.

6. It is concluded that Tofranil is of value in the treatment of less severe depressions, but in severe cases it would appear to be inferior to E.C.T. (Further comparisons between E.C.T. and Tofranil are indicated.)

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GROUP ADJUSTMENT AT THE SOUTH POLE

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INTRODUCTION

ANTARCTICA is the southern end of Earth, a continent surrounded by oceans. It is a land of extreme cold and it has been estimated that it contains over 85 per cent. of all the world's ice. It is larger than the United States and Europe combined but contains practically no vegetation or wildlife except along its coasts. The coldest temperature ever recorded anywhere in the world was noted here (minus 109.6 degrees Fahrenheit) and temperatures rarely go above freezing. The wind blows almost constantly, including 200 m.p.h. blizzards. In appearance, it is like a great white desert and it includes a cap of ice that in some places is 100,000 feet thick.

Following World War II the United States increased its Antarctic activities, and in 1954 began its support of the International Geophysical Year, during which scientists would gather data about that part of the world as well as the entire Earth. During Operation Deepfreeze I in 1955-56, the U.S. Navy established two stations on the shores of the Ross Sea, and then left a force of 166 men behind on the continent to winter over and to build new stations for the coming year. One of these stations was established at the geographic South Pole during late 1956, entirely supplied during its construction by aircraft operation. Named the Amundsen-Scott Station in honour of the only men who had previously set foot upon that spot, it was manned during the winter of 1956-57 by a force of 18 civilian scientists and Navy personnel. This group completed the construction of the base and gathered scientific information about natural phenomena at the Pole. This delegation was relieved in October, 1957 by a similarly composed group which carried on the work. They in turn were replaced in October, 1958 by another contingent of civilian scientists and Navy personnel under the leadership of Lieut. Sidney Tolchin, Medical Corps, U.S.N., and Mr. Julian W. Posey. Like the others, these men were completely isolated from the rest of the world for nearly 12 months. Once the Antarctic night set in they could not get to the outside world and help could not reach them even in the event of an extreme emergency. They existed entirely on the basis of their own supplies and resources until the winter was over. However, they lived in heated buildings, had plenty of good food, and were equipped with such gear as tractors, radios, and especially designed clothing. They also had such recreational devices as record players, ping-pong tables, and a small library. Nevertheless, they had to work extremely hard to maintain the station as well as to collect their scientific data. While they lived in relative safety and comfort, they had to be very careful not to make a small mistake that could be fatal. For example, the threat of fire was constant, and had a blaze destroyed their buildings they would surely have risked death from exposure. They had to limit their working time outdoors because of the danger of frostbite, and especially during the long night they had to be careful not to wander far from the station for fear of being lost in the white wilderness. Once the darkness

closed in, they were for all practical purposes confined to the limits of their indoor working spaces.

This paper is primarily an account of the experiences of this group, and how well they effected a psychological adjustment to this extreme environment of potential danger, hard work, and enforced confinement.

PROCEDURE

During the 12 months spent at the South Pole Station one of the authors (S.T.) gathered data on these eight civilians and nine military personnel. A personal diary was maintained which included a recounting of everyday events and detailed observations and introspections concerning the interpersonal relationships as they evolved at the station. All personnel were administered several psychological tests during the fourth and fifth months, and several of them took repeat tests during the 11th month of their stay. These tests included an Attitude Study, a List of Common Symptoms, a Group Behaviour Description Form, and a sociometric questionnaire known as a "Buddy Rating"* A complete medical examination of each man was made, and a record kept of his frequency of sick calls. A series of adjustment ratings of each man was also made by the military leader. Also available were the results of the psychiatric and psychological screenings examination conducted on each man prior to his leaving the United States.

RESULTS AND DISCUSSION

The Attitude Study is a list of items which required the person to indicate how much he agrees with a given statement or feels in response to a given question. For example, in response to the item "Are you bored?" he checks one of five answers, "almost always"; "usually"; "sometimes"; "rarely"; or "almost never". As seen by these answers to the questionnaire, most of the men expressed positive attitudes toward their existence at the Pole. The following results were noted:

All of the 16 men (one of the men did not fill out the forms) indicated that their present duties employed their abilities in the best way for accomplishing the mission of the expedition.

Twelve of the men indicated that they did not think the climate was dangerous to their health, while one man was sure that it was, and two others thought that it "probably was"; one man was undecided. It is interesting that those three men who thought the climate might be dangerous also ranked 3rd, 4th and 6th among the group in the frequency of sick calls made during the year.

Nearly all of them liked the food, thought the mission was important, thought the recreational facilities adequate, did not think that the time "passed too slowly", thought that the Navy had adequately provided for their physical wants, expressed acceptance of their leaders, felt that adequate medical care was available, and that they had been given adequate clothing.

Nearly all of them expressed the thought that they had close friends among the group, but showed a lesser acceptance of the idea that these same people would be as good friends under statewide conditions. There was also some hesitation in accepting the idea that everyone in the group respected everyone else, as noted by the fact that 14 of them either disagreed with or were not sure in response to the statement, "Everyone has a lot of respect for everyone else in my group."

* These tests were designed for Antarctic use by Dr. Herbert Zimmer of the University of Georgetown Medical School.

Five of the men indicated that they thought that their families were unhappy about the expedition, but all of them thought that as a result of it they would end up "ahead". They were all confident that they would return in good physical condition, but most of them showed concern that the amount of survival training given them might have been inadequate. Five of them indicated that more often than not they wished to be back in the States, and only one person indicated a wish for a longer stay in the Antarctic; but on the other hand, twelve of them noted a wish to go on another such expedition.

On the basis of these attitude-type test items it would thus appear that the group reflected generally high acceptance of their situation and of each other, even though there was also some realistic appreciation of the fact that they might have felt different toward each other under other conditions.

The Group Behaviour Description Form was constructed similarly to the Attitude Study except that the items reflected one's attitudes and beliefs about the group as a whole. In general, these responses indicated that they did a lot of "griping", had frequent bull sessions, that everyone did his share of work, that in the group there was generally good feeling, that rarely did anyone "rub anyone the wrong way" or that much bickering went on, that they could usually "have their own way", and that no one would eliminate anyone else from the group if given the opportunity. They felt that they helped each other when needed, that rank did not determine the privileges of the group, and that as a group they were accomplishing a great deal.

Upon closer analysis it is possible to see that this rather rosy picture of pleasant interpersonal relationships was less than perfect, however. For example, seven of the men felt that at least some of the men shirked their duties "sometimes", and four of these men felt that such shirking went on more often than not. While ten of the men responded that they felt that every man's opinion was valued as much as anyone else's, six of them were undecided and four of these latter men felt that seldom was each man's opinion as valued as the next person's.

But, on the basis of other criteria (sociometric test) it was found that of the four who indicated that job shirking was "usually" in evidence, three of them were among the six least liked people in the camp and the fourth one became a psychiatric casualty later in the year.

Of the six men who either were not sure or did not feel that each man's opinion was equally important, four were placed among the five least liked men in the camp (on the sociometric test), and a fifth one was rated by the military leader as consistently showing extremely poor adjustment to the group.

It would thus appear that those men who had the least acceptance among the group were perceptive of this fact and tended to reciprocate in kind.

The "Buddy Rating" was a standard type of sociometric test in which the men were asked to name the three men they "liked best", "liked least", who were "doing the best job", the "worst job", etc. Results tended to give further evidence that the group was not as cohesive and well integrated as the more direct questions of the Attitude Study and Behaviour Description Form would indicate. For example, there were five men who not only received one or no votes for being "Best Liked" but also received five or more votes for "Least Liked". While this did not especially mean that the group was openly opposed to these men, it did suggest that the group tended to have very few positive feelings for them. It is also striking that none of these five men were given any votes as "Doing the Best Job", while all of them received

at least one vote as "Doing the Worst Job" with four of them being near the top of the "Worst Job" list. Being well liked is apparently correlated highly with doing a good job.

At least three of the men could be described as "social isolates", receiving practically no "Like" votes, receiving more than five "Liked Least" votes, and at the same time showing either no preference for other members of the group (either refusing to completely fill out their forms or choosing "Like Most" persons as those who were extremely well liked by the entire camp and who did not return the subject's positive vote). In addition, these three men were described by the military leader as being among the poorer contributors to group morale and efficiency.

Three months after his return to the United States, the military leader was asked to rank each of the other 16 men according to how well they "adjusted to the group", placing the best-adjusted men at the top, the worse-adjusted at the bottom, the average man in the middle, and then ranking the others accordingly. Then, on the basis of the sociometric test the men were also ranked, being given three points each time they were chosen first as "Fitting in Best" with the group, two points for each time chosen second, and one point for each time chosen third. From this sum score was subtracted any points the man may have similarly acquired by being chosen as "Fitting in Worst" with the group. Any resulting ties were resolved in favour of the man who had more votes as being "Best Liked". This allowed for the 16 men to be ranked according to how their peers thought they fit in with the group. A Spearman rank-order correlation was computed for the two rankings and resulted in an ρ of $\cdot 83$ ($P < \cdot 01$).

It is, therefore, indicated that the two techniques of rating adjustment at the South Pole Station have a great deal in common and very likely measure similar things. However, since this correlation is considerably less than perfect it is also apparent that the two methods also measure different things.

Analysis of the sick-call records of the men revealed that the average number of calls made during the year for the Navy men was 20.0 while the average for the civilians was 8.25. The probable explanation for this difference is the fact that the physician was a Naval Officer and able to more easily persuade the military men to come to him for minor symptoms, while the civilians were much less indoctrinated to the use of "sick call" and at times had to be actively persuaded to allow the Medical Officer to attend them.

Age is apparently an important variable in such groups as the one under study. The average age of the group was 30.2 years, but it was found that those individuals who received low adjustment and performance ratings on both the sociometric test and the military leader's rating list were the youngest in the group, having an average age of about 22 years. In fact, when the sociometric scores are arranged into three groupings of "high", "middle" and "low", each member of the "low" group was under the age of 25 and each of the other members of the camp were above this age.

The factor of age was also noted in data gathered on a group of 38 men who wintered over at another station two years previously*. In this group it was found that those men who received low ratings from their supervisors included a higher number of men the age of 25 or below. Thirty-nine per cent. of the younger group received a low rating (3 or 4 on a four-point scale) while only 25 per cent. of the "older" men received such a rating. While these figures do not approach statistical significance, there is a distinct trend in evidence.

* Appreciation is expressed to Lieut. Michael Connery, M.S.C., U.S.N., for making these data available for analysis.

Because the personnel of the station were under two leaders there was some concern that this would be a source of friction between the military and civilian members. When a sociometric diagram is made from the number of "Best Liked" choices made by the men, there is seen to have been considerable "crossing over" of this hypothetical boundary. For example, on the "Best Liked" item military men were chosen by civilians a total of 9 times, while civilians were chosen by their military colleagues a total of 7 times. On the "Liked Least" item the military received 14 civilian votes while the civilians received 10 such votes from the Naval personnel.

However, on the "Doing the Best Job" item the military received 12 votes while the civilians were accorded no such votes, and on the item "if you had to be alone in the Antarctic with one other person who in your group would you choose?" the military were chosen 9 times but the civilians were picked only 3 times by the Navy men.

Part of the reason for the Navy men tending to choose themselves as doing the "Best Job" may be the fact that these choices were made at about the 4th month. To that date most of the work had been of the construction and maintenance type in which the Seabees excelled, and there had been less chance for the civilians to fully develop their scientific programme and thus demonstrate their own specialties.

It has been suggested by some Antarctic veterans that the Military-Civilian "split" is one that is really a split between the "intellectuals" or intellectually-oriented members of the group and the "non-intellectuals", or those who placed less weight on intellectual values, thus explaining why some of the men so easily crossed the Navy-Civilian "line".

In spite of the differences between the men which could be noted by careful analysis, the overall morale of this group may be said to have been excellent. While no objective data are available, considerable anecdotal evidence exists which suggests that this station had morale superior to that of most of the other small stations which have been maintained during Operation Deepfreeze.

Nevertheless, there were several things which seemed to contribute to episodes of poor morale among the men during the winter. Among these was the fact that some of the military men felt that they had been misled and given false information about what rewards might be given in return for their volunteering. For example, contrary to expectations, the men were not given their choice of duty following the Antarctic tour. The men were also under the impression that as each of them was considered for normal promotion in rate, they would not be subject to the usual "quota" restrictions, but as the promotion lists were released and received by radio during the winter it became apparent that such was not the case. Several of the men also related that they had been told by the recruiters that Deepfreeze volunteers would be given hazardous duty pay and other specified benefits. Not only did such rewards fail to materialize, but these men felt that the Navy did nothing to dispel such impressions even though there was ample opportunity to do so.

The question of extra pay was intensified when it was discovered that those military men who summered over for about three months at the main base at McMurdo Sound received an additional \$5.60 per day, while those who wintered over at the isolated stations for 12 months received nothing extra. The fact that a civilian technician with no more than a High School education might receive a ten to twelve thousand dollar salary for his year at the Pole was also a prime source of irritation to the Navy men.

The men were able to use the "Ham" radio in order to establish radio-

telephone contact with their families at home. However, because some individuals tended to monopolize the radio for this use there was some bitter feeling generated, and its regulation and use became a major item of concern for the leadership of the camp. The fact that the use of the radio for home contact was not an unmixed blessing was noted when several of the married men would become quite depressed after having spoken with their wives and children. One man was involved in trying to use the radio to forestall an impending divorce action by his wife, and more than once it was known that a wife in the States complained bitterly about the absence of the husband when he was needed at home in order to help handle some family crisis.

Other factors which seemed to affect morale included a felt lack of proper communication between the station, statewide activities, and other polar stations. When the main station at McMurdo Sound required that most messages should be routed through them, there was a lessening of outside radio contact. This was especially felt by the men when they did not feel free to "talk shop" with other small stations, and particularly by the civilians who felt that any interference with their communication with other scientists in the Antarctic and around the world was an unnecessary hardship.

Another morale factor was the seeming lack of co-ordination between the civilian and military indoctrination programmes. In this instance, as in others, the military leader and civilian scientific leaders met for the first time after they had arrived on the Ice just prior to their final embarkation for their own particular station. It was also found that the civilians had been given no exposure to their military "station-mates" prior to departure to the Antarctic and were given no indoctrination as to their exact roles in the "two-group" situation. It was felt by some members of the station that the civilians were under the assumption that the Navy personnel would do all of the housekeeping chores during the winter and that the civilians would have no responsibility for other than their own scientific projects.

However, since this was not compatible with the realities of polar living, considerable effort had to be expended in order to alleviate the morale-destroying attitudes which arose out of this misunderstanding.

It is desirable, of course, to be able to select beforehand those men who will be compatible with their wintering-over colleagues. When the men were screened for this duty they were given psychiatric interviews and some psychological testing in order to weed out the obvious psychiatric risk. They were interviewed by a psychiatrist (in some cases two), interviewed and given a Rorschach Test by a psychologist, and given a final rating established by collaboration of the psychiatrist and psychologist. The screeners were required to label those men who were psychiatrically unfit for stressful duty, a task in the traditional vein of placing a man somewhere on the normal-abnormal continuum of mental health. They were also required to evaluate each man's ability to "adjust" to this kind of duty, or to be "successful" or "unsuccessful" in performing with the group with which he was to be confined. This latter task is obviously much more difficult and constitutes having to pick from among "normals" those who possess special interpersonal skills which are peculiarly relevant to this particular kind of duty. Under this system about 10 per cent. of the volunteers have been rejected in recent years.

An attempt was made to see if the psychiatric and psychological ratings made prior to each man's selection had any significance for predicting his "success" in a group such as was confined at the South Pole.

While each psychiatrist and psychologist was asked to place the man on

a five-point scale none of the 16 men studied in this group (screening data was not available for one man) were rated as either "not acceptable" (point one) or "outstanding" (point five); nor were any of them given a unanimous "2" or "4".

Because the rating scores of these 16 men were so restricted in range, it was necessary to analyse these data differently in order to discover if the screening procedures had any positive relationship to the criterion of group adjustment.

The verbatim reports of the screening clinicians, that is, their description of the dynamics and traits evidenced by each man, were given to five judges who were not associated with the screenings. These judges, all experienced clinicians, were asked to evaluate each personality description and decide if it contained positive, negative, or neutral implications for prediction of Antarctic adjustment.

A scoring system was developed whereby each time a man received a "positive" rating from one of the five judges he was given a score of 3; each time he received a "neutral" rating he was given a score of 2; and each time he received a "negative" rating he was given a score of 1. Thus, if a man received five "positive" ratings his score was 15; if he received five "negative" ratings his score was 5. A Spearman rank-order correlation was computed between these 16 scores and the overall score on the Buddy Rating, producing an ρ of .48 ($P < .05$).

This latter finding is consistent with another study by Weybrew* in which it was found that the correlation between a self-report of adjustment for 100 men at the larger station (McMurdo Sound, with about 130 men) and the screening ratings (using the previously mentioned 1-5 scale) was .38.

It would therefore appear that the psychiatric-psychological screenings to which the Operation Deepfreeze personnel are now subjected not only serve to identify those men who are grossly disturbed but also have some ability for predicting compatibility and group efficiency while in Antarctic isolation. It seems reasonable that additional effort would be justified in conducting a careful investigation of just which variables in the Deepfreeze screening procedures are thus predictive of group adjustment.

SUMMARY

Psychological test data and personal observations were gathered concerning 17 men who wintered over at the geographical South Pole. From these data it became possible to sketch the general attitudes of the men toward the Antarctic and each other; to describe some of the source of group tension and morale; and to discriminate between the most successful and least successful members of the group.

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THE MEASUREMENT OF MOTIVATION THROUGH THE USE OF OBJECTIVE INDICES

By

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and

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WHILE abilities and attitudes are frequently subjected to measurement, motivation has largely escaped the attention of experimental psychologists and psychometrists. Attempts to use subjective and interpretive techniques have not met with any notable success, and it would seem that this whole area represents one of the most neglected fields of psychology, in spite of its obvious major importance both from the fundamental and from the applied points of view. We would argue that the objective measurement of motivation or drive in human beings is an essential requirement for clinical psychology, and we would further suggest that such objective measurement can only be accomplished in terms of some theoretical system in which drive has a recognized place and mode of interaction with measurable variables. It has been suggested by Eysenck and Maxwell (1961) that only the Hullian system provides such an account, and some evidence is to hand (Kimble, 1950; Wasserman, 1951; Claridge, 1960; Eysenck and Maxwell, 1961) to reinforce the view that deductions can be made from this system which will generate testable hypotheses relating to the measurement of motivation.

According to the theory in question, massed practice (e.g. on the pursuit rotor) produces reactive inhibition (I_R), which grows as a linear function of duration of practice. I_R is regarded as a negative drive which cancels out part or all of the positive drive (D) active in the testing situation. When $I_R = D$, performance stops and an involuntary rest pause (I.R.P.) ensues. During this I.R.P. inhibition, being a fatigue-like product, dissipates, and when I_R falls sufficiently below D , performance begins again. I_R accumulates again until another I.R.P. is enforced, and once the critical level has been reached where I.R.P.s interrupt performance, these rest pause, will occur regularly, until a lengthy programmed rest pause allows I_R to dissipate completely. This dissipation is shown in performance as *reminiscence*, i.e. an improvement in performance when massed practice is resumed after the rest pause, as compared with the level of performance just preceding the rest pause.

The sequence of events may be shown diagrammatically as in Figure 1, where minutes of practice are plotted on the abscissa and amount of drive on the ordinate. The line slanting across the figure represents the growth of I_R for a low drive group (L) and a high drive group (H), respectively. It will be seen that for the low drive group (whose drive for the purpose of demonstration is assumed to be equal to 0.5 units) the critical level where I.R.P.s occur is reached after two minutes; I_R never grows beyond this level, as it can never exceed the value of D . The high drive group, whose drive level is arbitrarily

* We are indebted to the Society for the Investigation of Human Ecology for a grant which made this study possible.

assumed to be 1.50 units, continues to show a growth of I_R until the critical level is reached after six minutes of practice; no further growth of I_R is possible after that point.

It will be clear from what has been said that reminiscence is a good measure of I_R , provided the programmed rest pause is sufficiently long to allow all or nearly all of the I_R to dissipate. But I_R is a direct measure of D at and after the moment when the critical level has been reached. "It follows that reminiscence, in these circumstances, may be regarded as a direct measure of motivation or D and it can be predicted that high drive levels should give rise to greater reminiscence scores than low drive levels" (Eysenck and Maxwell, 1961).

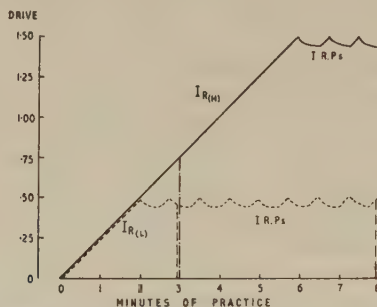


FIG. 1.—Diagrammatic representation of the growth of reactive inhibition (I_R) in low (L) and high (H) drive groups after varying amounts of practice. Involuntary rest pauses (I.R.P.s) arise when $I_R = D$.

In an experimental study of this hypothesis, these authors chose high and low drive groups totalling 120 Ss. Both groups were given massed practice on the pursuit rotor, time-on-target being summed for each successive 10-second period. Half the Ss practised for three minutes, the other half for eight minutes; a six-minute rest pause followed practice, and this in turn was followed by another four-minute post-rest practice period. (The first post-rest trial was preceded by two seconds of practice, in order to make this trial properly comparable with the last pre-rest trial.) Reminiscence score used was: first post-rest trial minus last pre-rest trial. The reminiscence scores for the short practice groups were 0.80 (high drive group) and .54 (low drive group). For the long practice groups, the reminiscence scores were 1.51 (high drive group) and .51 (low drive group). Analysis of variance disclosed that differences in length of practice were significant at the 5 per cent. level, differences in drive strength were significant at the 1 per cent. level, and the interaction was significant at the 2 per cent. level. These values are in line with the hypothetical state of affairs shown in Figure 1, and strongly support the general hypothesis.

The experiment to be reported now represents a duplication and extension of the Eysenck and Maxwell study. It can be seen that after two minutes of practice there should be *no* difference in the reminiscence scores of the high drive and low drive groups because at that point the low drive group is only just beginning to produce I.R.P.s. To test this hypothesis the two-minute practice period was chosen as one of the variables in the new experiment. The other period chosen for the massed practice preceding the rest pause was six minutes because, as will be seen from Figure 1, it is at this point that I.R.P.s should begin to occur for the high drive group provided that the growth of I_R is linear and passes through the zero point on the abscissa. If this were true, then

reminiscence scores for the high drive group should be equal after six and after eight minutes of massed practice.

A total of 148 Ss was used in the experiment, half of whom were tested under low drive motivation, the other half under high drive motivation. As in the previous experiment, all the Ss were young industrial apprentices, 16 to 18 years of age; all were male. The high drive group took the test as part of an entrance examination for engineering apprentices at one of the biggest car-making firms in England; they were under the impression that the score on this test would play some part in determining their success or failure in the examination. The Ss in the low drive group had already been accepted as apprentices, and were working with the firm in question; under Trade Union agreement they were guaranteed advancement regardless of how well or how poorly they worked, so that they had no particular motivation to do well on the pursuit rotor test, which they were told was of experimental interest to psychologists only.

Each group was, in turn, subdivided into two, differing with respect to the amount of pre-rest practice; each of the final four groups consisted of 37 Ss. The long-practice groups received six minutes of practice (thirty-six 10-second trials), while the short-practice groups received two minutes of practice (twelve 10-second trials); this was followed by a six-minute rest period, and this, in turn, by a six-minute post-rest practice (thirty-six 10-second trials). (The reason for having the post-rest practice two minutes longer than in the previous study was to enable us to obtain further evidence on the role of motivation in determining performance as such; this point will be discussed later. In all other respects the experimental treatment of the groups was identical with that in the first experiment, so that reminiscence scores are fully comparable.)

RESULTS

The performance curves of the high drive and low drive groups respectively are given for the two-minute pre-rest practice groups in Figure 2, and for the six-minute pre-rest practice groups in Figure 3. Time-on-target is shown on the ordinate in each case. It will be seen that in no case are the groups differentiated with respect to performance, a point which will be discussed in detail later on.

Reminiscence scores were calculated as before for the four groups and are

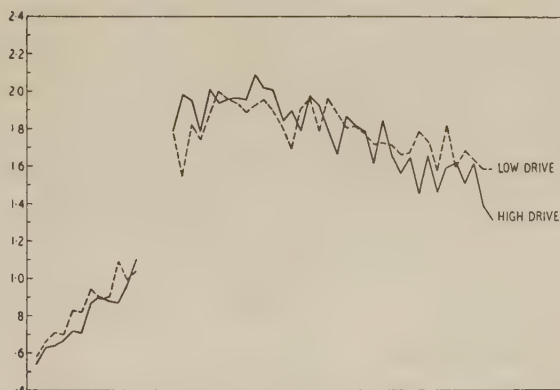


FIG. 2.—Performance on the pursuit rotor of high and low drive groups during two minutes preceding and six minutes succeeding a six-minute rest pause.

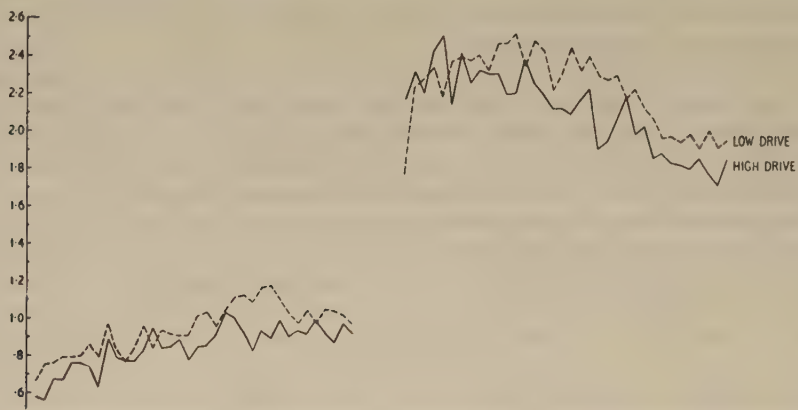


FIG. 3.—Performance on the pursuit rotor of high and low drive groups during six minutes preceding and six minutes succeeding a six-minute rest pause.

given in Table I. It will be seen that as predicted, the high drive long practice group has a higher mean reminiscence score than the low drive long practice group. It will also be seen that as predicted, there is no difference between the short practice groups in response to high or low drive.

TABLE I						
Drive					2 Minutes	6 Minutes
Low	..	..	..	..	.72	.84
High	..	..	..	..	.68	1.29
Mean	..	..	..	..	.70	1.02

A variance ratio test of the significance of the differences between the values in Table I leads to Table II, part 1.

TABLE II, PART 1						
Source of Variance				Sum of Squares	d.f.	Mean Square
Between groups	..	..	..	8.8754	3	2.9585
Within groups (error)	..	..	..	115.1989	144	.8000
				124.0743	147	

Although the variance between the means of the groups is not large (7.15 per cent. of the total) the number of observations is sufficient to demonstrate its significance: $F=3.70$ and $.05>P>.01$.

The exceptional mean, as predicted, is that of the group with high drive and long practice. The sum of squares of the variance between groups can be broken down as in Table II, part 2. The variance between the means of the

TABLE II, PART 2						
Source of Variance				Sum of Squares	d.f.	Mean Square
Variance between the high drive, long practice group and the remaining groups	..	..	..	8.4082	1	8.4082
Variance of the remaining groups among themselves	..	..	..	.4672	2	.2336

remaining groups has a smaller m.s. than the error variance in Part 1, so a general mean of $\cdot 75$ can reasonably be derived for the three. This is also the expected mean for the high drive, long practice group, on the assumption that the conjunction of high drive with long practice has no specific effect on reminiscence. The large variance between the mean for the high drive, long practice group, $1\cdot 29$, and the general mean for the remaining groups, $\cdot 75$, with its variance ratio $F=8\cdot 4082/\cdot 8000=10\cdot 51$ and $P<\cdot 01$, shows the significance of the specific effect observed in this experiment.

Figure 4 shows in diagrammatic form the combined results of the present experiment and the one reported by Eysenck and Maxwell (1961). It will be seen that the low drive groups have reminiscence scores which between the limits of the experiment (from two minutes of massed practice to eight minutes of massed practice) remain constant at a level of about $\cdot 65$. The reminiscence scores of the high drive group, between the same limits, exhibit a linear increase from the two-minute period where they equal with those of the low drive group,

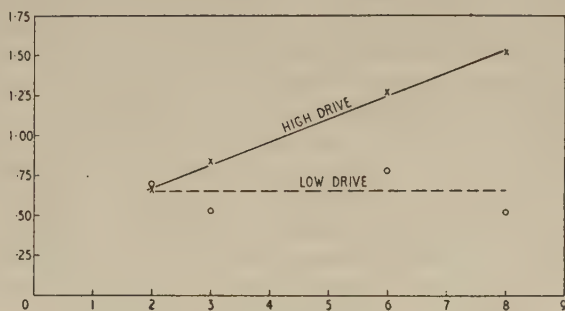


FIG. 4.—Reminiscence scores of high and low drive groups plotted as a function of the number of minutes of massed practice preceding the rest pause.

to the eight-minute period where they are between two and three times as high as those of the low drive group. The rise in score of the high drive group between six and eight minutes is unexpected and, if confirmed, would invalidate the hypothesis that the increase in I_R with practice is linear; extrapolating the results from the high drive groups backwards in a linear fashion, it will be seen that the ordinate at the point of zero practice is cut at a level considerably above zero, which is nonsensical. It seems possible, therefore, that the growth of I_R below the point represented by two minutes' practice is *curvilinear*, or else that it is linear but has a different *slope* from growth after the two-minute practice period. Which of these hypotheses is true must remain a problem for further research. So must the question of the level at which the high drive group accumulates enough I_R to equal D ; it is impossible from the data presented, to be sure that no further growth would occur with longer practice periods than eight minutes. It seems safe to conclude, however, that for the purpose of measuring motivation or drive, relatively long periods of practice are optimal; to have a period of practice which does not enable the high drive group to reach the point where I_R equals D , leads to a minimization of the difference between it and the low drive group.

DISCUSSION

In one sense, it may be said that the data here presented constitute a justification of the writers' belief in the value of the Hullian theory in making possible

the measurement of motivation. It is unlikely that in the absence of this theory, and its development by Kimble (1950), reminiscence would have been used in this manner. In another direction, however, our results throw some doubt on the general applicability of the Hullian formula for performance as a function of drive and habit, i.e. ${}_sE_R = f(D \times {}_sH_R)$. According to this very widely accepted formulation, which is accepted even by learning theorists otherwise critical of Hull, we would expect that performance on the pursuit rotor would be better for the high drive than for the low drive group. In the experiment by Eysenck and Maxwell (1961), it was already found that "the evidence for differentiation effects of drive on performance is rather poor. While such differences as are observed are in the expected direction, and achieve significance at one point, yet the total differentiation is not very impressive." In the present experiment this differentiation is even less impressive; indeed, on the whole the low drive group does, if anything, better than the high drive group, although the differences do not, at any point, approach significance. Reasons for this failure of drive to influence performance may be many and various; only one suggests itself as a likely explanation. The tasks used by psychologists for the measurement of performance may be assigned a position on a continuum, ranging from subject-paced at one end to experimenter-paced at the other. In an experimenter-paced task, the subject, if he co-operates at all, co-operates fully, and differences in drive beyond the minimum required for co-operation do not have much chance to affect performance. In a subject-paced task, however, such as the five-choice serial reaction time experiment (Venables, 1959; Claridge, 1961), where the subject's reaction automatically produces the next task, motivation has a good opportunity of affecting the speed of performance at all levels. There is little evidence to support this hypothesis, but it is in line with the theory presented here that on the pursuit rotor, extraverts have equal performances with introverts, but higher reminiscence scores (Eysenck, 1960a), while on the five-choice serial reaction time test extraverts have poorer performance but equal reminiscence scores (Claridge and Herrington, 1960). This may be interpreted in terms of the general theory that extraverts accumulate more inhibition than do introverts; under experimenter-paced conditions, as on the pursuit rotor, this excessive inhibition shows itself in greater reminiscence scores. Under subject-paced conditions, as in the five-choice serial reaction time test, this excessive inhibition shows itself in a generally poor performance, leading to equal reminiscence scores (i.e. inhibition is being dissipated throughout the performance period). This hypothesis, of course, is highly speculative and should not be recorded as being based on a firm footing; nevertheless it suggests some interesting research possibilities.*

It will have been noticed that in our experiment we have paid no attention to individual differences among Ss. These are quite considerable, as shown by the high variances, and it is possible that there may exist considerable differences between Ss with respect to the degree of motivation elicited by a standard set of experimental tasks and instructions. This could easily be investigated by correlating reminiscence scores of a given set of Ss working under equal drive-inducing conditions on a variety of different tasks. It would not be unexpected if positive correlations were found between reminiscence

* This explanation is not adequate by itself to account for the failure to find differences in performance due to drive; it is likely that recourse will have to be had also to the Yerkes-Dodson Law. It would be idle to indulge in further speculation at this point; clearly duplications of this experiment with different tasks are required to provide the factual basis for theoretical discussions.

scores on these different tasks, at least as long as these tasks were all located towards the experimenter-paced end of the continuum. It is certainly notable that widely different tests of persistence are usually found to correlate together (Eysenck, 1960b), and there is an obvious similarity between persistence and high drive. This whole problem, however, is likely to be complicated by possible differences in drive between extraverts and introverts, neurotics and normals, or other groups which may be used for experimental purposes. Even when there are no differences directly attributable to say, extraversion, interaction effects are still a possible source of complication (Eysenck, 1957; Eysenck, 1960a).

One further complication that should be mentioned is due to differences likely to arise in studies with certain types of abnormal Ss. Low reminiscence scores are evidence of lack of drive *only when the rest pause involved is long enough to ensure complete dissipation of I_R* . In a number of unpublished studies we have found that psychotics, in particular schizophrenics, showed complete absence of reminiscence on the pursuit rotor under conditions where normal Ss and neurotics have never failed to show reminiscence. It would be tempting to interpret this finding in terms of lack of drive on the part of our schizophrenic Ss, but an alternative hypothesis was elaborated stating that the rate of dissipation of I_R in psychotics may be abnormally slow, so that a 10-minute rest pause, while sufficient for normal and neurotic Ss to dissipate practically all the I_R accumulated during rest practice, did not suffice with the schizophrenic groups to dissipate more than a minute fraction of the I_R accumulated. In a critical experiment, normal and psychotic groups were tested with 10-minute and with 24-hour rest periods, and while the longer rest period did not increase the reminiscence scores of the normal group, results with the schizophrenic groups showed a lack of reminiscence after 10 minutes' rest but a reminiscence score even greater than that of the normals after the 24-hour rest period. This example will make it clear that the use of reminiscence scores as measures of motivation requires a very careful theoretical analysis, particularly when experiments are planned dealing with abnormal Ss. It is, nevertheless, our opinion that such experiments will be all the more fruitful because of the firm theoretical underpinning which the use of reminiscence scores as measures of motivation receives from modern learning theory.

SUMMARY

Certain deductions from modern learning theory have been outlined according to which reminiscence on the pursuit rotor should constitute a good measure of motivation. Groups of young male industrial apprentices were tested under conditions of high and low drive, and the results confirmed predictions made on the basis of this theory. No differences were found with respect to performance between high drive and low drive groups.

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THE DAY HOSPITAL APPROACH IN CHILD PSYCHIATRY*

By

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HISTORICAL BACKGROUND

THE Day Hospital approach in adult psychiatry appears to have begun with the original conception of Cameron in Montreal. He stated (Cameron, 1958) that both the original concept and its further development sprang from growing appreciation of the possibilities of new methods of treatment and diagnosis: at this time the rapid rise of the cost of in-patient treatment took place and there was increasing realization of the importance of the treatment milieu. At the same time as Cameron was setting up his Day Hospital in Montreal, Bierer was setting up the Marlborough Day Hospital in London (Bierer, 1951) and he discussed the theory and practice of Psychiatric Day Hospitals later (Bierer, 1959). Harris (1957) also discussed the implications of Day Hospitals and Night Hospitals in psychiatry with regard to the adult patient.

The most extensive developments of this method of approach in adult psychiatry have taken place in Great Britain where over thirty units are in existence at the present time. These day hospital developments have been reviewed by Freeman (1960) who quotes Moll (1958) as dividing Day Hospitals into five classes:

1. An integral component of a general hospital.
2. Affiliated with a hospital, but in a separate building.
3. Part of a community service or out-patient department.
4. Within the grounds of a mental hospital.
5. A completely separate treatment centre.

These developments of the Day Hospital approach in adult psychiatry are reviewed in detail by Farndale (1961).

This approach had not been applied in a continuous and structured manner as an integral part of a child psychiatry service (so far as the writer is aware) until the Day Hospital at "Tiverlands" was started some three years ago. The interest this method of approach aroused at various meetings, including the Child Psychiatry Section Meeting of the Royal Medico-Psychological Association in Newcastle of 1959, and the World Health Organization Seminar on Child Guidance and Child Psychiatry held in Brussels in August, 1960 (when the writer presented a brief outline of this approach) has suggested that a general presentation of its development, principles of practice, aims, and three years' experience may be of interest and value in future planning. Apparently a Day Hospital for child psychiatric problems is soon to be opened in Paris, and this approach has been operating in Aarhus, Denmark, for the past year in conjunction with an in-patient Child Psychiatric Unit.

This paper is not intended to be more than a general presentation of the work. Detailed presentation of the type of cases admitted, methods of treatment and results obtained will be submitted in a further communication.

* Based upon a paper read to the Child Psychiatry Section of the Royal Medico-Psychological Association on 7 May, 1959.

THEORETICAL CONSIDERATIONS

It is possible to formulate several clearly defined theoretical advantages of the Day Hospital approach in child and family psychiatry. These can be summarized as follows:

1. *Attendance*

It avoids the dilemma facing most child psychiatry out-patient units, involving the choice of a maximum of one therapeutic interview or group session a week or admission to residential units if the weekly session is not sufficient. The Day Hospital can provide a *flexible* situation, in which attendance can range from one morning or afternoon a week through one day, two days . . . up to five days a week.

2. *Treatment*

- (a) Avoidance of the need to remove a child to a residential establishment eliminates the separation trauma and the difficulties of re-establishment in the home, following a residential placement (which nearly always means placement in a unit far away from home under a different set of psychiatrists, psychologists and psychiatric social workers, who have the greatest difficulty in maintaining close contacts with the home).
- (b) It avoids placement of the child in a residential establishment not really suited to his needs, owing to the lack of adequate in-patient units. In particular, many children may not need to be sent to special schools for mal-adjusted children, where adequate psychiatric, psychological and social work facilities are often lacking.
- (c) The Day Hospital provides a therapeutic environment which can be adjusted under psychiatric direction, to meet the individual and group needs of the patients attending. A therapeutic environment of great *flexibility* is thus provided.
- (d) Continuity of handling and of contact with parents, schools, local authorities, probation officers, child care officers, etc., can be maintained, and therapeutic manipulation of the home and school environments can be effected on a sound basis.
- (e) There is the possibility of providing for children who are not suitable for individual psychotherapy and individual play therapy treatment which can be based upon the manipulation of the environment, in terms of the particular history of faulty handling and training received at home or elsewhere. In this situation, treatment of character disorders may be attempted by such a manipulation of the Day Hospital environment and of group dynamics.

3. *Investigations and Diagnosis*

The Day Hospital situation avoids the necessity of basing diagnosis upon the somewhat artificial individual interview with the child or upon short-term group observation. More continuous, longer term observations of the child can be made in an environment which can be manipulated by the production of stresses, useful in assessing the child's adjustment potentialities.

If the Day Hospital is part of a General Hospital out-patient service, special departments needed in investigation are readily available. In particular,

departments of child health, pathology, radiology, neurology, neurosurgery and electro-encephalography may be contacted.

The effects of drug therapies (particularly tranquillizers and anti-convulsants) can be easily assessed, as can the effects of other methods of physical treatment.

4. *Planning of Services*

Should the Day Hospital approach be found successful in treating some severely disturbed children, the call on in-patient child psychiatric beds would be diminished (there is a gross deficiency in such facilities at the moment and there are waiting lists of over a year in many units); the calls for placement in residential special schools for maladjusted children would be diminished. These facts would have far-reaching implications upon the future national planning of child psychiatry facilities and special educational facilities.

5. *Training*

A *flexible* training situation for psychiatrists, psychiatric social workers, psychologists, nurses, teachers, etc., would be available.

6. *Research*

A *flexible* research situation would be present in which, in particular, the effect of methods of treatment, other than individual therapy from a psychiatrist or play therapist, could be studied and assessed.

DEVELOPMENT OF THE DAY HOSPITAL

It must be admitted that the foregoing considerations were *post hoc* so far as the Day Hospital at Tiverlands was concerned. There were no special facilities in the field of child psychiatry in Tyneside, which has a population of about 1,250,000, when towns within a radius of twenty-five miles are included (excluding Sunderland, which has run a Local Authority Child Guidance Service for more than twenty years). This tragic lack of facilities in the North-East was due not so much to the disinclination of the Regional Board and Local Authorities to set up such services, but to abortive attempts to do so, on account of lack of trained psychiatrists, psychologists and psychiatric social workers.

There are, of course, no residential facilities for psychiatrically disturbed children, either in terms of in-patient beds or of special schools or hostels for maladjusted children in the North-East.

In order to cut across this situation, the Regional Board, in association with the Department of Psychological Medicine, King's College, University of Durham, set up an Out-patient Unit in a converted private house as part of the services provided by the Newcastle General Hospital, which also contains out-patient and in-patient departments of psychological medicine for adults, the latter comprising some fifty-four beds. The aims of this new unit for children were to establish a limited clinical service, to provide teaching for undergraduate and post-graduate (University of Durham D.P.M.) students, for psychiatric social workers and others, and to carry out research, which is an indispensable part of the work of a teaching unit.

Tiverlands was opened in late June, 1957, when the writer came to Newcastle to take charge of this section of the Joint Department of Psychological Medicine (which also includes the out-patient, in-patient and adult day hospital

facilities at the Royal Victoria Infirmary) under the overall charge of Professor Martin Roth.

The staffing of Tiverlands, at that time, consisted of the medical director, a senior psychologist, a secretary, a nurse and staff for cleaning who attended until 11.30 a.m. The building itself contained a kitchen, three large playrooms (one of which has a one-way screen), a small garden, a self-contained garage and a brick coal shed.

Soon after the clinic opened, a child was referred who was very disturbed: his symptoms included spitting because he was afraid his saliva might do him harm if he swallowed it; fear of electricity coming from the electric light switches, fear of gas coming from the gas fire, etc. Out-patient individual therapy was ineffective, and did not begin to meet his needs. Placement in an in-patient unit elsewhere was impossible because of long waiting lists. Rather than continue ineffective treatment as an ordinary out-patient, we took him on as a day patient. Soon after this, a child with gross disturbance of behaviour, involving neurotic and aggressive behaviour, was also taken on as a day patient after weekly psychiatric interviews proved ineffectual.

Both these children improved to such an extent with Day Hospital attendance that they were re-established in their normal school environments and were much more settled at home. It seemed, therefore, that the Day Hospital approach might be well worth using as a permanent method in the unit.

During the past three years the staffing of the Unit has been built up to the level shown in the appendix. This staffing is still deficient in some areas. All members of staff are employed by the hospital authorities except the teacher, who is employed by the local department of education.

IMPLICATIONS OF THE ABOVE STAFFING

The provision of the above staff tells its own story in terms of our experience at Tiverlands. It must be stressed, however, that the staff are not solely engaged with Day Hospital patients. Far from it. The Day Hospital is a somewhat theoretical concept so far as Tiverlands is concerned, in that it is not physically separated from the out-patient clinics, offices, etc. The staff are involved in assessing and treating some 270 new cases a year and deal with some 6,600 out-patient attendances, if the day patients are included, excluding school visits made by the psychologist (150 a year) and home visits made by the psychiatric social workers (300 or more a year).

In the first instance, so far as the day patients were concerned, a good deal of individual attention was available from psychiatrists, but as the work of the clinic and the case-load built up, less and less of this time was available. The Day Hospital patients increased in number, and Day Hospital "treatment" began to be prescribed for those patients who were thought to be suitable, but very little time was given to the nursing and other staff by the psychiatrist in charge, to discuss the handling of these patients. This situation arose "by default" due to pressures on the clinic rather than by deliberate planning, and it therefore became necessary to make formal provision for meetings with staff to discuss the day patients. An hour to one and a half hours a week are now given for this purpose.

With the appointment of a senior registrar in May, 1960, it also became possible to provide more satisfactory day-to-day contact between the medical and nursing staff. Thus, after three years, we are beginning to be in a position to manipulate the investigatory and therapeutic environment in terms of the real needs of the children and staff.

The provision of a full-time teacher (who will be a teacher in the widest sense of the word, not necessarily engaged in full-time formal teaching) is an indication of the need we have found to arrange a more structured environment within the Day Hospital setting, and the need to establish subgroups of the sixteen Day Hospital patients.

The need for staff of suitable personality and interests has also been made quite clear, and this has been solved by making the appointments to Tiverlands direct to the Unit instead of appointing the nurses as part of the joint department of psychological medicine on a rotating basis.

It will be clear from the foregoing that in the first instance the Day Hospital included an individual therapeutic situation with patients attending for the day. As the Unit developed, it became a therapeutic environment with little, if any, individual therapy from the medical staff and at first, with little contact between the medical and nursing staff; then came the present phase, which provides a more co-ordinated and medically supervised *flexible* environmental, therapeutic and observational situation which can also include some individual psychiatric treatment.

THE THERAPEUTIC ENVIRONMENT

It is difficult to describe in clear terms the therapeutic environment of a Day Hospital. Both physical factors, the building, play-rooms, and so on, and personal factors come into play. The latter are the vital attributes of the therapeutic environment, particularly with regard to the personality of the nurses, domestic staff and other staff in the Unit.

The physical aspects include the three play-rooms, the garage (shortly to be equipped as a construction workshop), the coal-shed (now converted into a room with lighting, heating, sink unit, and a drained floor, which will be used for clay pottery work and messy play) and the front and back gardens.

All children meet for lunch in the downstairs playroom, which is near the kitchen. Meals are brought down in heated containers from the main hospital some three hundred yards away, and dished up in the kitchen at Tiverlands. Tea is provided on the spot and all children meet for tea at 3.15 p.m. These meals provide two definite meetings of all children during the day.

The environment can, in general terms, be described as the result of an attempt to provide a warm accepting climate to which children, whatever their behaviour or disability, will respond. In fact, we do not see anything like as many severe outbursts of temper, violence, or destructive behaviour as would be expected from the history of the children's behaviour before attending the Day Hospital. This finding may well be a measure of our success in providing the optimal emotional climate, and testifies to the understanding and tolerance of the staff, at all levels.

The requirements of the Unit as an out-patient clinic have to be borne in mind, and a certain amount of restriction of noise and activity has to take place. The presence of a school on each side also constitutes a limiting factor.

The nursing staff meet a member of the medical staff once a week for discussions on individual cases or general topics concerning the handling of children, or for more formal instruction concerning orientation and basic facts of child psychiatry.

Each nurse is allocated about three or four children who are to be under her special care. She is required to observe these children and to write weekly notes about such observations of the child's progress. Subgroupings under the charge of individual nurses can be provided.

Sister is in day-to-day contact with the senior registrar, and weekly staff meetings are held on Friday afternoons to discuss the progress of some of the day patients.

The medical director and the teacher meet once a week to discuss the groupings of children attending her, and to make sure that she is not asked to take children who are not ready for a more formally structured group situation. The teacher keeps weekly notes of each child's progress and attends the staff conferences, so that there is full liaison and feed-back of observational material.

PRACTICAL CONCLUSIONS

The general administrative and practical conclusions we have reached can be summarized as follows:

1. There is a need to provide a warm accepting environment, in which immediate problems of behaviour can be dealt with as they arise by the medical staff, either by individual interview or by advice to the non-medical staff involved.
2. The number of children who can be handled in the Day Hospital will depend upon the type of problem the child shows and upon the number and personalities of the staff available, as well as upon the physical structure of the Unit.
3. Regular staff meetings should be held at least weekly if the Day Hospital patients are not to become the "lost legion", and provision must be made for one member of the medical staff to be available to the sister in charge for emergency consultation as she requires.
4. Some structure is needed in the environment, which should not be regarded as a "free play" centre, since this is disturbing both to some of the timid, fearful patients and to the aggressive children, who may be adversely affected by a continuous discharge of aggression.
5. The importance of maintaining contact with the mother, and sometimes the father, of children attending the Day Hospital, so far as these parents can accept this contact, has been clearly demonstrated to us.
6. It is not possible to run a Day Hospital for children without making liberal use of the hospital car and ambulance services, since it is unreasonable to expect mothers with other family and domestic commitments to travel to and from the Unit twice a day, and since the children themselves cannot travel alone on public transport. To insist that children should be brought by the parent may lead to refusal of therapy or the building up of adverse attitudes in parents, which will vitiate the treatment being given to the child.
7. There is a need for an accurate and efficient Day Hospital attendance register, so that failures of attendance can be immediately followed up. Thus absence from clinic and from school without the knowledge of the clinic staff can be avoided, and the ensuing difficulties in relationships with the child, parents and Local Education Authority can be obviated.
8. There must be ample playroom accommodation and ample equipment, as well as out-door facilities.
9. There must be access to a meals service to provide lunches, and a kitchen in the Day Hospital to provide morning milk, afternoon tea, etc.
10. There is a need for a Day Hospital waiting list, and a special meeting once a week between the psychiatrist in charge and sister in charge, in order to

discuss whether a proposed new admission to the Day Hospital will fit in with the group already attending. Too many aggressive children will disrupt the group, particularly if delinquency is a feature of their symptomatology. On the other hand, too many shy and inhibited children may lead to inadequate group pressures, and group stagnation.

THE RELATIONSHIP BETWEEN THE DAY HOSPITAL FOR PSYCHIATRICALLY DISTURBED CHILDREN AND THE DAY SCHOOL FOR MALADJUSTED CHILDREN

The question of the relationship between the Day Hospital and the Day School for maladjusted children is important and needs clarification and definition. The essential differences are as follows:

1. The Day Hospital provides observation and short-term treatment facilities for the psychiatrically disturbed child, with close supervision and manipulation of the "therapeutic environment" by the psychiatrist, who is always available. The Day School for maladjusted children provides a longer term, less flexible environment, with some psychiatric supervision and psychologist participation, usually on an infrequent part-time basis.
2. The accent in the Day Hospital is on "treatment of children's illness" in a flexible therapeutic environment, many of the children being too ill to attend even a special school, whereas the accent in the special school is on education for special needs, in which formal teaching plays a much more prominent part, and the environment may be much more rigid.
3. The aim of the Day Hospital is to get the children well enough to attend their ordinary schools, or to attend a special boarding or day-school for maladjusted children if necessary.
4. The "treatment" provided in the Day Hospital includes close contact with the parents, and with the ordinary schools in the case of some children who attend them on a part-time basis, or who are likely to return to them. Thus therapeutic efforts are brought to bear on the family and school, as part of the treatment programme, by the Day Hospital team. This is not usually provided for by the special school for maladjusted children, and never to the degree possible in the Day Hospital approach. (The writer also holds an evening clinic in the out-patient department, to help parents and adolescents who cannot attend the child psychiatry unit during the day.)
5. The Day Hospital requires ready, and sometimes emergency, access to special medical facilities. These are not so easily available in the Day School for maladjusted children.
6. The Day Hospital provides continuity of handling and treatment and is open during the school holidays.

FUTURE DEVELOPMENT AT TIVERLANDS AND IN NEWCASTLE

It is apparent to all of us working in Tiverlands, that the presence of day patients within the out-patient clinic is far from ideal, because of the need to limit the general noise and mobility of the day patients for the sake of the ordinary out-patient clinics. Optimally, the day hospital should be specially designed, and situated in a separate wing of the out-patient clinic, with physical separation of the two functions. Ideally, also, there is a need for a close attachment to an in-patient unit for children who fail to respond to the Day Hospital approach.

A Day Hospital, suitably designed as such and attached to an out-patient and in-patient unit, is part of a plan submitted to the Regional Hospital Board and is being considered as a high priority by the Board.

The therapeutic deficiencies in the present unit can be summarized as follows:

1. There is need for provision of more individual psychotherapy and play therapy for those patients attending the Day Hospital who would benefit from this combined approach.
2. The need for in-patient accommodation.
3. The need for a co-ordinated Local Authority service of hostels and schools for maladjusted children, so that a graded approach from hospital or clinic to special school or hostel and then back to ordinary school and family life can be effected for those children who need such a gradual and planned approach. These are likely to be the most seriously disturbed children, whose progress can be retarded or halted by placements which are too abrupt. It is hoped that in the course of time these deficiencies will be made up. Indeed, the first of these is likely to be partially met by a re-organization of the Unit's efforts in the next few months.

WIDER IMPLICATIONS OF THE DAY HOSPITAL

The fact that seriously disturbed children have been shown to benefit from an environmental approach without individual specialist treatment is a challenge which may have far-reaching implications in the future planning of children's services and endorses the theoretical remarks made earlier. It is bound up with the general questions of natural remission of symptoms, the relationship of symptoms to personality development and the whole problem of character disorders.

It is, however, essential that well-planned research be carried out in suitably staffed pilot Day Hospital units, if any conclusions of definitive value in future planning are to be formulated.

The field of child psychiatry will be faced for many years to come with the problem of training and provision of professional staff. Any methods which can be shown to be of definitive value, involving the use of less highly-trained staff than the psychiatrist, psychotherapist and play-therapist, will be of national importance and the immense cost of providing specialist services in the field of childhood mental and emotional disturbance may well be diminished.

SUMMARY

1. Theoretical aspects of a Day Hospital approach in child psychiatry are formulated.
2. The development of a Day Hospital for psychiatrically-disturbed children in Newcastle upon Tyne is described.
3. Practical implications and future developments in the light of the three years' experience of this Day Hospital are discussed.
4. A comparison is made between the Day Hospital for psychiatrically-disturbed children and the Special Day School for maladjusted children.
5. Some implications for future national planning and for research into methods of therapy other than individual treatment by a psychiatrist or psychotherapist are mentioned.

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APPENDIX

*Present Staffing of Combined Out-patient and Day Hospital Child Psychiatry Unit—
 "Tiverlands"*

Psychiatrists

The Medical Director (9 sessions a week). Consultant Psychotherapist (2 sessions a week). Consultant Psychiatrist (1 session a week). Assistant Psychiatrist—S.H.M.O. (whole-time). Senior Registrar (whole-time).

Psychologists

Senior Clinical Psychologist (whole-time). Clinical Psychologist (whole-time).

Psychiatric Social Workers

2 Psychiatric Social Workers (whole-time). 2 Psychiatric Social Workers (part-time)—7 sessions in all.

Nurses

1 Sister (whole-time). 2 Staff Nurses (whole-time). 1 Auxiliary Nurse (whole-time). 1 Nursery Nurse (whole-time).

Teachers

1 Teacher (full-time, provided by the Local Education Authority).

Clerical and Secretarial Staff

1 Clerical Worker (operates switchboard, makes appointments, organizes filing and indexes, etc.). 2 Shorthand Typists (for psychiatric social workers and psychologists). 1 Shorthand Typist (for medical staff).

Cleaners and Domestic Workers

2 Cleaners from 7 a.m. until 11.30 a.m. 1 Cleaner from 12 noon until 5 p.m.

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EXISTENTIALISM AND PSYCHIATRY*

By

FRANK FISH

IN psychiatry we often use different kinds of psychology without being aware of doing so. Before we consider existentialist psychiatry in detail we must discuss the three different kinds of psychology, namely, explanatory psychology, understanding psychology and interpretive psychology.

In explanatory psychology, we investigate human and animal behaviour scientifically and endeavour to find rules and laws.

In understanding psychology, we feel ourselves into the situation of other people and understand how one psychological event follows another. For example, we see a man whose face and general demeanour indicate that he is angry. We feel ourselves into his situation and understand why he is angry. We all, of course, practise this simple understanding psychology.

On the basis of understanding psychology we can erect psychological theories and use these theories to interpret behaviour. Interpretive psychologies are mixtures of naïve understanding psychology and concepts drawn from philosophy, neurology, neuropsychiatry and even folklore. In the Western world Freudian psychology is the most popular interpretive psychology whereas in the Soviet Union neo-Pavlovian psychology plays the same role. Personally, in my day to day practice of psychiatry, I find Freudian psychology extremely useful. It helps me to understand and use in psychotherapy the material which my patients communicate to me.

Using Heidegger's philosophy, Binswanger has produced a new psychopathology which he calls *Daseinanalysis*. This, I believe, is really yet another variety of interpretive psychology. Other psychiatrists have, of course, used existentialist concepts. However, in the limited time at my disposal, I will only be able to give a brief account of Heidegger's views and of Binswanger's analysis of schizophrenia. This is, therefore, a discussion of only one aspect of existentialist psychiatry and not an extensive review.

Heidegger wishes to answer the question, "What is the Being of things which are?". Whereas traditional philosophy looks beyond the physical in its search for the essence of Being, Heidegger considers that the human existing being, the *Dasein* (the Being who is there) is the true source of being. This word "*Dasein*" comes from the two German words—*da*, meaning there, and *sein*, meaning to be or being, since the *gerund* in German has the form of the infinitive.

The *Dasein* (the existing human being) stands in the world and this "standing-in" can be understood as based on the human concern for things. There are three aspects of concern and each is a mode of "standing-in". These modes may be authentic or inauthentic. In the authentic mode the *Dasein* has full insight into what he is and relates himself to things with this in view. In the inauthentic mode the *Dasein* is lost in the necessities of everyday life and his relationships to things do not take into account his total possibilities.

* Lecture given to the Section of Psychiatry, Birmingham Medical Institute, on 11 October, 1960.

The three authentic modes are:

1. The discovery of self as already in the world (Befindlichkeit).
2. Understanding (Verstehen).
3. Discourse (Rede).

The corresponding three inauthentic modes are:

1. Ambiguity (Zweideutigkeit).
2. Curiosity (Neugier).
3. Prattle (Gerede).*

Just as past, present and future are inseparable modes of time, so each of the modes of "standing-in" cannot be separated from the other two.

Inauthentic existence is due to Dasein being wrapped up in his concern for everyday life. This everyday state of the Dasein is *Verfallensein*, the state of being fallen. In this stage, true understanding is impossible and ambiguity occurs. The lack of understanding leads to a concern for trivialities which is the mode of curiosity. Dasein expresses this curiosity in prattle, i.e., the results of an inauthentic existence are expressed in prattle.

The three authentic modes of "standing-in" are based on an ontological structure which can be regarded as the basic attitude of the Dasein. This is *Sorge* (Care). In order to be authentic the Dasein must understand the structure of his own existence. He must achieve a state of care in which he realizes that he alone is responsible for meaning in the world and in which he realizes his own nothingness as a finite being.

To achieve this, a special act of self-discovery, which throws the whole of Dasein's being-in-the-world into question, is necessary. The first step in self-discovery is *Angst*, a nameless dread which leads the Dasein to ask, "Who am I?" and "What am I doing here?" *Angst* arises from a psychological shock or from disgust produced by an utterly inauthentic existence, which leads to a realization of the Dasein's *Unheimlichkeit* (Un-home-li-ness) in the World. The *Angst* produced by this leads to self-questioning and the discovery of the whole structure of one's own existence.

In *Sorge* (Care) the Dasein discovers himself in three ways:

1. As being on the way, i.e., projecting himself into the future. This is Understanding (Verstehen).
2. As already being in the World. This is the discovery of self. (Befindlichkeit).
3. As being with things, i.e., living in a reciprocal relationship with the things that are. This is Discourse (Rede).

We now have to consider the temporality of Dasein. He lives in expectation of the future and the only sure thing in the future is death. When the Dasein comprehends absolutely his future death he realizes at the same time the whole of his future existence. Dasein is thus Being-towards-Death (*Sein-zum-Tode*) and when he realizes this he is free to engage himself and decide his attitude towards things.

The Dasein begins in a state of thrown-ness (*Geworfenheit*) and ends in

* It is interesting to note here the verbal tricks of this philosophy. *Rede* (Discourse) is the authentic mode and *Gerede* (Prattle) is the inauthentic mode. This verbal contrast is lost in translation.

death so that he is always haunted by his own Nothingness. The Dasein is the only creature who can accept the idea of his own Nothingness and is therefore able to accept or reject his own destiny. From out of Nothing the Dasein inserts a disengaging nothing which allows the things that are to be understood. In other words, Dasein opens a space within the horizon of time in which the light of Being can reveal itself.

Dasein must accept his guilt due to his previous inauthentic existence by means of resolution which is "an extraordinary mode of the open-ness of Dasein. . . ." This resolution produces an authentic existence and allows the Being-with-Others (Mitsein).

Heidegger does not concern himself very much with Mitsein (Being-with-Others), Sartre rejects Mitsein and deduces the existence of the Other from the Look. Binswanger has stressed this mode of Being and has modified Heidegger's views. He considers that Love is the ontological opposite of Care and that Being-with-Others (Mitsein) is the basis of Love. Love creates an Eternity and vanquishes space, time and history since the loved one exists as a Thou for the loving one and therefore exists after his death. This is merely a verbal trick. It is difficult to see how the Dasein can be eternal as an image in the mind of the Mitseinde (co-existent) who loves him. Surely the image disappears when the loving co-existent dies or even dement. Binswanger has in fact grossly distorted Heidegger's views by shifting the focal point of the philosophy from the predicament of the isolated individual to the difficulties of the social individual.

Time for Heidegger is dependent on the Dasein who projects himself into the future by authentic resolution. Since he anticipates his potentialities, his future is partly a now. This is different from the everyday view of the future which regards it as a state of possession of things, which has not yet arrived. The Dasein can possess his past now when he realizes he has been thrown into the world and is Being-towards-death. Dasein must assume responsibility for what he was and is. The present for the authentic Dasein is the full Moment (Augenblick) when he makes the past present by a future which he possesses as a resolute act of projection into the future. This concept of the full Moment or Augenblick comes from Kierkegaard who was a manic-depressive. One wonders if this was not originally based on Kierkegaard's experiences in mania. However, the morbid origin of a concept does not mean that it is untrue.

The modes of "standing-in" take place in authentic or inauthentic temporality. Understanding depends on projecting resolution into the future. If this is not done, then the future is merely a now which is not yet. Self-discovery is determined by the past which is acknowledged and its possibilities are used for future projections. The Dasein who exists in authentic temporality lives in the Moment (Augenblick) in which there is a new light on the things which are. This new Being is realized in the form of a discursive unveiling of the new reality. This Discourse (Rede) is expressed in speech. Words play an important role in Heidegger's philosophy. He and his followers attempt to understand concepts by analysing words. As German is a synthetic language, this artificial philological analysis is easy to carry out. Apart from this analysis of words, they invent new words or use established words in an unusual way.

It is now necessary to consider how this philosophy has arisen and how it stands in relation to other philosophies. There are two basic positions in philosophy, realism and idealism. The realist or materialist believes that mind is a product of matter and the sensations are produced by matter. The idealist regards mind as primary and matter as secondary, so that sensations are not entirely produced by a material world which is independent of mind. An extreme

variety of idealism is solipsism in which the individual is only sure of his own existence and considers his sensations to be present in his imagination.

Kant produced an interesting variety of idealism which has had a profound effect on modern European thought but especially on German philosophy and psychology. He suggested that we perceive things as phenomena but we do not perceive things as they really are, namely things-in-themselves. To know a thing as it really is we must make a transcendental leap from the thing for us to the thing in itself. Thus human experience consists of the elaboration of perception so that the mind orders the perception of objects in time and space.

From this it follows that there are two types of psychology, one which deals with mental phenomena and the other which deals with the transcendental mind. The mind in this latter sense cannot be investigated but only analysed by means of the *a priori* concepts which are present in the mind. If one follows this kind of philosophy one is entitled to erect one's own concepts about the mind and organize them into a system without any need to appeal to empirical facts.

This is what Heidegger does when he tries to derive the way in which the human being exists from pure contemplation of the problems of the existent in terms of philosophical ideas. He imposes an order on things, which is designed to lead to the discovery of the things-in-themselves. He reaches the conclusion that by means of self discovery one is able to detach oneself from the everyday aspect of things, i.e., from the Kantian phenomena. This detachment creates a distance from things which allows of their proper appreciation. This self detachment is expressed as "the inserting of a nothing". Thus the understanding of things in themselves is achieved. The weakness of this whole system is the lack of an adequate method of testing the results. How do we know that we really have discovered our true selves or that our projections into the future are consistent with our potentialities and the situation? Despite Heidegger's protests one cannot help feeling that his philosophy is really a variety of solipsism.

The most striking feature of this philosophy is its morbid subjectivism. Dasein is not "born of woman" but is thrown into the world. He is not trying to live honestly in harmony with his fellows but is living towards death. This is the type of thinking which one finds in the shut-in shy adolescent who finds it difficult to make interpersonal relationships. In his utter isolation he asks himself "Who am I?" and "Where am I going?" and often receives an answer like that of Omar Khayyam.

"A Moment's Halt—a momentary taste
Of Being from the Well amid the Waste.
And Lo! the phantom Caravan has reach'd
The Nothing it set out from—oh, make haste!"

It is therefore not surprising that ideas resembling existentialist thinking are to be found in disturbed adolescents and young intelligent schizophrenics.

So much for a psychiatric criticism of the philosophy; now we must consider its application to the problem of schizophrenia. Binswanger has published detailed existential studies of five schizophrenic patients during the years 1944 to 1953. In 1957 he republished these studies in book form under the title of *Schizophrenie* and in his introduction to this book he summarized his findings.

In his investigations he was looking for a daseinanalytic order and structural order of the schizophrenic Dasein. He tried to grasp the special structures of the disposition of Being. Instead of disease entities he claims to have discovered a homogeneity of definite structure and developments of the Dasein.

He claims that the basic concept in the understanding of the schizophrenic development of the Dasein is Inconsequence of or the breaking apart of natural experience which normally proceeds as a natural sequence of connections. Heidegger expresses this as our Abode (Aufenthalt) with things and considers that it consists in our allowing the things that are to be as they are. Binswanger's patients wanted to dictate to things and force them to be what they wanted them to be. Since practical experience could not unfold freely, gaps appeared. In most cases this Inconsequence could be found in the behaviour in childhood.

This Inconsequence of practical experience leads the patient to find ways out which will restore the lost order and fill in the gaps. No satisfactory way out can be found and Dasein finally develops a desire for finality, i.e., for an end. The last way out is found in a high flown or verstiegene ideal which gives the patient a base from which he can continue the struggle. However, the high flown ideal is inadequate for the life situation.

The inconsequence of experience is split into an alternative; a rigid Either-Or. A new order seems to have emerged in the form of the high flown ideal but high flown-ness offers no way out. In fact the high flown ideal is merely one side of the alternative. The opposite side cannot be completely suppressed but an attempt is made to cover it up. The existential alternative is expressed in some field of activity such as the body or social life. Thus in one patient it was thinness and fatness of the body while in another it was the choice of being socially superior or inferior.

This insoluble alternative leads to the Dasein being in a harassed state. It culminates in a renunciation of the antinomic problem and a withdrawal of the Dasein from the accomplishment of his existence. In the case of Ellen West there was a free decision to withdraw from life by ending it. In the case of Jürg Zünd, there was a withdrawal from social life and the patient chose life in a mental hospital. In other cases there was a withdrawal from the Dasein into the completely involuntary mode of delusion. In this mode the Dasein does not voluntarily renounce actual living or social life but renounces life as an independent self and surrenders itself to foreign powers.

In the case of Ilse, she burnt her hand in order to demonstrate her martyrdom to her coexistents. After this she developed the delusion that she was being put on show. Thus the active behaviour of making a show of herself changed into passive suffering of being put on show. In the mode of martyrdom there was a split in the sequential connection of practical experience but not a complete destruction. The martyrdom did not permit of any development of coherent practical experiences which then broke apart giving rise to an empty place. This was filled by a completely inconsequential mode of experience, namely that of being a sacrifice to others, which had some inner connection with earlier experiences. The Dasein became stuck fast in a model of practical experience which modelled all new experiences according to its own model.

Thus the act of sacrifice in this case forms a connecting link between normality and delusional psychosis. In another case the connecting link was an almost delusional superstitious oracle which consisted of the patient extracting meanings from the world by means of a play on words. The resignation of the Dasein here took the form of the renouncing of her own decisions in favour of those of her enemies.

Thus delusion is one of the forms of resignation of the Dasein in relation to its antinomic tension produced by inconsequence of practical experience. Withdrawal from actual living or from social life are other forms of resignation. In delusion there is a resignation in the form of a withdrawal from the cohesion

of one's own decisions and a complete self surrender to the power of others. The indications which the delusional patient detects around him are the secondary or tertiary results of the resignation of the Dasein in the form of taking oneself out of one's own cohesion of decisions. The content of the delusion depends on the way in which the Dasein fills out the gaps created by his withdrawal from the antinomic problem of practical experience. He draws on his fantasies for material to fill these gaps.

In 1957 Binswanger published a book on *Three Forms of Failed Existence*—*Verstiegenheit, Verschrobenheit, Manieriertheit*. Kahn has translated these terms as eccentricity, queerness and stiltedness, but I feel that either they must be left in the German as technical terms or translated into English neologisms. *Verstiegen* comes from the verb *versteigen* which means to climb the wrong way or to go astray on the mountain. This has led to the figurative use of *verstiegen* to mean high flown or eccentric. *Verstiegenheit* is thus high flown-ness. *Verschroben* comes from the verb *verschrauben* which means to screw up or screw the wrong way. *Verschrobenheit* is usually translated as queerness, eccentricity or crankiness but could be translated as screwiness. *Manieriertheit* is usually translated as mannerism in the ordinary nontechnical sense of the word but in order to avoid confusion with the symptoms of mannerism I suggest it be translated as manneredness.

Binswanger considers that High flown-ness, Screwiness and Manneredness are modes of failure of the Dasein in the sense of the historical movement of the Dasein having come to an end, or being stuck fast. Consequently, Dasein is not able to be with others in love and friendship. These modes are very close to the modes of schizophrenic Dasein such as "stiffness" and being "being-at-stand-still". *Verstiegenheit* (high flown-ness) is an anthropological disproportion. The normal Dasein moves in a horizontal and a vertical plane and his movements in these planes are correlated but in *Verstiegenheit* the Dasein has climbed too high or become stuck in the vertical dimension. In *Verschrobenheit* (screwiness) the Dasein is transverse or oblique to the situation and his fellow existents because he justifies his behaviour not by relating it to the situation and his co-existents but by a dominating idea which arises from his fallen state. In *Manieriertheit* (manneredness) the Dasein carries on a discussion with himself instead of with others. His existence is split. Binswanger believes that *Verstiegenheit, Verschrobenheit* and *Manieriertheit* are existentially related to schizophrenic inflexibility, stiffness and splitting.

Stiffness means that the movement of the Dasein into the future is cut off. The *verstiegene* (highflown) ideal and the *Verschrobenheit* are initial stages of schizophrenic stiffness. Thus in *Verschrobenheit* (screwiness) the obliqueness of the patient's world makes authentic self foundation incomplete. In *Manieriertheit* (manneredness) there is an incessant repeating of the reflexion of one's own Dasein in the mirror of the pattern or model of Man. The mimicry and gesture of the individual become a mask and it is as if there were an iron net around all expressive movements. The free play of Dasein's movement is lost.

Binswanger summarizes the relationship of the three unsuccessful forms of existence to schizophrenia as follows:

"The proximity of schizophrenia to the forms of Dasein of *Verstiegenheit, Verschrobenheit* and *Manieriertheit* and the passing over of these forms into schizophrenia from time to time depends therefore on the fact that these forms represent intermediate existential forms between the authentic historical movement of the Dasein and the complete arrest of this movement. These are the forms therefore which somehow make it possible for Dasein to hold his own in

the world for a variable length of time although he is not in the homeland of love. This does not occur in the sense of the success of Dasein with the development of his freedom, his abundant possibilities and his creative powers but in the sense of remaining on the edge of the abyss which we call the "emptiness" or "stiffness of psychic life", "schizophrenic thoughtlessness, breakdown or deterioration".

After all this, have we learned anything new about schizophrenia? Such ideas as a break in the coherence and sequence of experience are not new and do not seem any more profound when expressed in Heideggerian jargon and neologisms. *Verstiegenheit*, *Verschrobenheit* and *Manieriertheit* are merely long-winded ways of saying that a given person is odd. We know some people who are odd develop schizophrenia and some do not. It is difficult to see what is to be gained by calling oddness "an intermediate form of existence between the authentic historical movement of the Dasein and the complete arrest of this movement".

Perhaps Binswanger's criticism of a patient who wrote books can be applied to Binswanger himself. He writes as follows about this patient whom he cites as an example of *Verschrobenheit*.

"Apart from the inconsequent consequence of the mode of communication in the sense of the Dasein as *verschroben*, yet another essential characteristic which is connected with *Verschrobenheit* is especially clear in this case. This is the type and manner in which the Dasein here handles a precise totality of circumstances. The deficient consideration for the coexistents, even the denial of the basis of communication in general, brings it about that the totality of circumstances which is in question—i.e., the whole of the historical doctrines and methods of the time—are not preserved in their richness but are restricted or reduced to a purely formal scheme of thought".

This is precisely what Binswanger himself has done. He has reduced the richness of schizophrenic symptomatology to a simple scheme. The philosophers' stone of schizophrenia, the basic psychological disorder from which all symptoms are derived, is presented to us in a new disguise. Nothing new is said which can help us to understand or explain schizophrenia.

This criticism of Binswanger applies equally to Heidegger. He has denied the basis of communication and grossly misused the German language. This is well expressed by the Russian existentialist theologian, R. Berdyaev, who writes:

"Heidegger has, ironically enough, rationalized the Kierkegaardian theme into a rigid and almost scholastic system. He puts a genuinely existential experience into the strait jacket of rational categories, which are really quite unfit for it and, in so doing, conjures up a whole inventory of almost unbearable and incomprehensible terminology, the only virtue of which is its undoubted originality."

When one examines Heidegger's philosophy rationally, it becomes obvious that it is an attempt to convey some type of mystical experience. It is in fact a religion without a god. It is no more justifiable to apply Heidegger's ideas to clinical psychiatry than it is to apply the ideas of Roman Catholicism, Buddhism or Islam. This view gains some support from Karl Jaspers who has approached the matter from a different angle. He calls existentialist psychiatry "a philosophical error".

In some ways Daseinanalysis and allied schools of psychiatry are a natural development of German psychiatry. Griesinger's slogan, "Mental disorders are brain disorders" was the inspiration of the careful investigation of psychiatric

symptomatology carried out by the German psychiatrists of the late nineteenth century and the opening of this century. Kraepelin went farther than most of his colleagues and used experimental psychological techniques in his investigations. He was, of course, a pupil of Wundt, the founder of experimental psychology. However, although this psychology has flourished in Britain and America, it has been overshadowed in Germany by the most speculative varieties of psychology, which are more philosophical than scientific. This, I believe, has led to the arrest in the progress of German psychiatry. Thus the only recent developments have been the application of speculative psychology to clinical problems.

The careful description of psychiatric symptoms and the delimitation of psychiatric syndromes was completed many years ago by the classical German psychiatrists. The syndromes and symptoms were then interpreted in terms of theories which were usually borrowed from speculative psychology. The different schools used different psychological theories. A large number of polemical articles were written but no one ever thought of deciding the issues by experimental investigation.

Today the tendency is to turn away from the old sterile disputes and to accept some variety of interpretive (or dynamic) psychology. Thus neo-Freudian and existentialist views have become popular in Germany and the classical clinical psychiatry is now being neglected. The baby is being emptied out with the bath. I believe that the findings of German classical psychiatry are of great value since we can use them to delimit the conditions which we are investigating. On the other hand, the philosophical psychological constructs are usually hardly worth translating into English and in fact many are untranslatable since they are more connected with the nature of the German language than with reality. This particularly applies to Daseinanalysis.

To sum up, I am certain that there is nothing of additional value in existentialist psychiatry. Formulation of our patients' problems in a new jargon is an interesting scholastic exercise but the time spent in this way would be more profitably spent in experimental investigation.

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Reviews

Narrative of a Child Analysis. By MELANIE KLEIN. Hogarth Press, London. Price 75s.

This is a long book of 466 pages, which gives a very detailed account, session by session, of four months' analytic work with a ten-year-old boy and includes reproductions of numerous drawings. The child's behaviour and conversation and the analyst's interpretations are given with accompanying descriptive notes and theoretical conclusions from the material, and it is intended as a companion volume to Mrs. Klein's *Psycho-Analysis of Children*, in order to illustrate her technique in greater detail. It gives a valuable illustration of the type of child analysis developed by Mrs. Klein and her followers, which differs considerably in theory and technique from the orthodox Freudian child analysis as practised in this country and the United States. Mrs. Klein, for example, makes no use of the interpretation of defence, a cornerstone in ego-analysis, but places much greater emphasis on the interpretation of most of the material in terms of the transference.

The writing is clear and readable and provides a vivid account of the analysis, but the wealth of the material, much of it of necessity repetitive, makes it a difficult book to read. It is essentially a book for the specialist, and the novice will be well advised not to attempt using the technical tools here demonstrated without an adequate training in the theoretical considerations from which they are derived.

SUSANNAH DAVIDSON.

Mental Subnormality. By W. A. HEATON-WARD. John Wright & Sons, Ltd., Bristol. 1960.

This little book gives a brief account of the causes and degrees of mental subnormality, with a short section on the developing child, mental tests, mental illness in the mentally subnormal, epilepsy and community and hospital care. The relevant provisions of the Mental Health Act are outlined.

The choice of the terms "primary amentia" and "secondary amentia" is unfortunate, as this terminology is outmoded, and a phrase such as "simple undifferentiated amentia" should not be used in 1960.

The section on hospital training is also somewhat old fashioned, mentioning domestic work and residential domestic posts for females and farm training schemes for males. There is no reference to the rapidly growing facilities in hospitals for training in light industrial work, subcontracted from factories, such as making cake boxes, assembling radio parts, toy making and scores of other jobs of a simple repetitive type for which there is wide scope for subsequent employment in the community.

The book may be of some value to those requiring a quick orientation in this field.

B. W. RICHARDS.

The Integrity of the Personality. By ANTHONY STORR. Heinemann, London, 1960. Pp. 174. Price 15s.

The author of this short and intriguing book is a consultant psychiatrist and analytical psychotherapist of the Jungian school. His aim is to define in simple terms the basic assumptions upon which his practice of psychiatry and psychotherapy rests. In this he has succeeded, and the personal and subjective quality of his writing combined with the clarity of his formulations make this a refreshing and readable document, the more so because it concerns an area where "scientific" and "objective" are still too frequently equated. Dr. Storr hopes that his book may contribute towards understanding of the psychotherapeutic relationship; in the sense that he has conveyed vividly and sympathetically some important principles about the dynamics of the

psychotherapeutic process and the meaning of psychotherapy within the framework of Jung's idea of "individuation", his hope is justified. There must be few books that have both the width of perspective and the grounding in clinical practice.

Despite this, its appeal is likely to be extensive, rather than intensive. This does not mean that it is superficial, but rather that the width of the perspective that the author has chosen precludes any serious examination of the sort of problems that occupy psychotherapists and are of the most interest from the point of view of research and future understanding. Thus, practising analysts are likely to be disappointed by a statement such as "... the degree of recovery which takes place in the patient is proportional to the degree of maturity of the relationship which he is enabled to make with the therapist". From the point of view of the patient at the end of a successful analysis this is important and true, but many analysts would claim that the really interesting and important changes which can occur are those which seem to be proportional to the degree of immaturity of the relationship that the patient is able to make with the therapist.

The author, however, is well aware of the importance of the therapeutic regression as a "reculer pour mieux sauter", and the real problem is the level of the audience to which the book is addressed. The informed layman should find it outstanding, but might have a little trouble in negotiating some of the technical aspects of projective identification. Psychiatrists may value it most for the clear way in which Jung's prospective view of mental illness is presented. The positive attitude to neurosis as a frustrated attempt at self-realization; the compensatory function of the unconscious; the view that many symptoms are not evils to be banished so much as signposts to mental contents that are clamouring to be integrated; these are amongst the topics that Dr. Storr discusses very well. His own views on the significance of incest, the relationship of dependency to aggression and the meaning of fascination are of considerable interest. He finds Fairbairn's theories valuable and makes use of them in his task of delineating the factors that he believes are common to all schools of psychotherapy. It is a pity that his perspective also precludes some study of the way in which the Jungian approach seems to differ quite widely from other schools. The archetype concept is mentioned briefly, but there is no reference in the index to either symbol or archetype.

One of the best chapters deals with the difference between analytic and non-analytic psychotherapy, and between analysis and indoctrination. This is of particular interest in the light of the late Professor Kennedy's recent observations on the subject.

MURRAY JACKSON.

Trifluoperazine, Clinical and Pharmacological Aspects. Introduced by HENRY BRILL, M.D. Published by Lea & Febiger, (U.S.A.), 1958 and Henry Kimpton, (London), 1959. Pp. 219. Price 26s.

This "monograph" on Trifluoperazine (Stelazine) consists of 25 reports from the United States and Canada on various aspects of the drug. The book contains useful information on the pharmacological properties of the drug, its side-effects and toxic reactions and about the management of the latter.

It is difficult to accept uncritically the conclusions of the many clinical reports regarding the high efficacy of the drug in various psychiatric and organic disorders, because on the whole, the reports are impressionistic and do not reach the standard of precision and scrutiny which one expects in these days with drug trials. Some of the illustrative case reports, however, are informative.

N. H. RATHOD.

Trance in Bali. By JANE BELO. Oxford University Press, London, 1960. Price 60s. net.

In the 1930s Jane Belo lived in Bali for eight years working for part of the time with Margaret Mead and Gregory Bateson. She became familiarized with all aspects of Balinese culture, and this book records her detailed study of the complex set of trances in existence at that time. This is a unique study of an institution that is rapidly changing, though despite the disturbances produced by the War, the ceremonies

persist. This book describes the people who practise the trances. They were interviewed before and after the trances, and the dissociative nature of the states clearly shown. The actual trances were closely observed on film, and by note taking by three Balinese secretaries against a time scale. The descriptions of these trance states account for the bulk of the book. The performance of the various types of trance were scheduled in accordance with the religious life and were a means whereby the Gods of Good and Evil could be communicated with, thereby relieving communal anxiety. This function of the trance ceremonies is particularly stressed.

While the trances are considered to be hysterical in character, the "players" are not regarded as hysterics. It is interesting that only certain individuals in that culture "who differed from their fellow villagers by a high emotionality, a lively imagination and, in general, a somewhat infantile psychic structure" were capable of going into a trance. Yet psychological tests given to a group of trancers and non-trancers revealed no differences between the groups, and it was concluded that the trancers could not be classed as "abnormal" personalities.

The book makes interesting general reading, reminding the reader of the wide range of dissociative phenomena that can be "normal" within a particular cultural setting, while for the specialized reader the detailed description of the various trance states remains unique.

B. M. DAVIES.

Psychiatrie. By KURT KOLLE. 5th edition. Georg Thieme, Stuttgart, 1961. Pp. 480. Price DM 29.80.

This is an excellent text book, written mainly for medical students and for general physicians—practitioners and non-psychiatric specialists. The size of the book does not frustrate this aim because a good deal of space is devoted to illustrating its points by lively descriptions and explanations for the uninitiated reader. It is precisely this which makes the book particularly readable—more so than a shorter but more compact text could be.

Kolle occupies the chair in Munich once held by Kraepelin. His views are firmly rooted in the teachings of the Heidelberg school, but the book is by no means one-sided. Where divergent views exist he presents them without succumbing to the temptation of a superficial eclecticism. He writes "Jaspers gave psychiatry, which like any other science, has to become aware of its own methodology, its proper place in the social sciences . . . Had Jaspers been better understood the attempt for a methodologically clear synthesis should have begun much earlier . . . Experimental psychology born under the star of the natural sciences was for a time overrated . . . Psychology turned more and more towards the psychology of meaning which . . . under the influence of Freud was introduced into Psychiatry as a method in its own right. Psychoanalysis grew into depth psychology and as such found its link with the cultural traditions (*Geistesgeschichte*) . . . Unhampered by the growth of advances in the natural sciences and free from naturalistic preconceptions the clinician can now once again concern himself with psychology as such . . .". (Translated by the reviewer.)

The book is carefully kept up to date with every new edition and has so far avoided the malign tendency of so many text-books to grow fatter in the process. In fact the present edition is slightly smaller than the previous one.

J. HOENIG.

Memory, Learning and Language. The Physical Basis of Mind. Edited by WILLIAM FEINDEL. University of Toronto Press, and Oxford University Press (London), 1960. Pp. 80. Price 16s.

This is a little book on a large topic containing a half-dozen essays of varying merit; most of them are very elementary—one is rather technical—but none make any new contribution, nor review old work in any new, stimulating manner.

The contributors include, Drs. W. Feindel, A. Hoffer, J. F. Leddy, W. Penfield, A. Porter, J. W. T. Spinks.

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